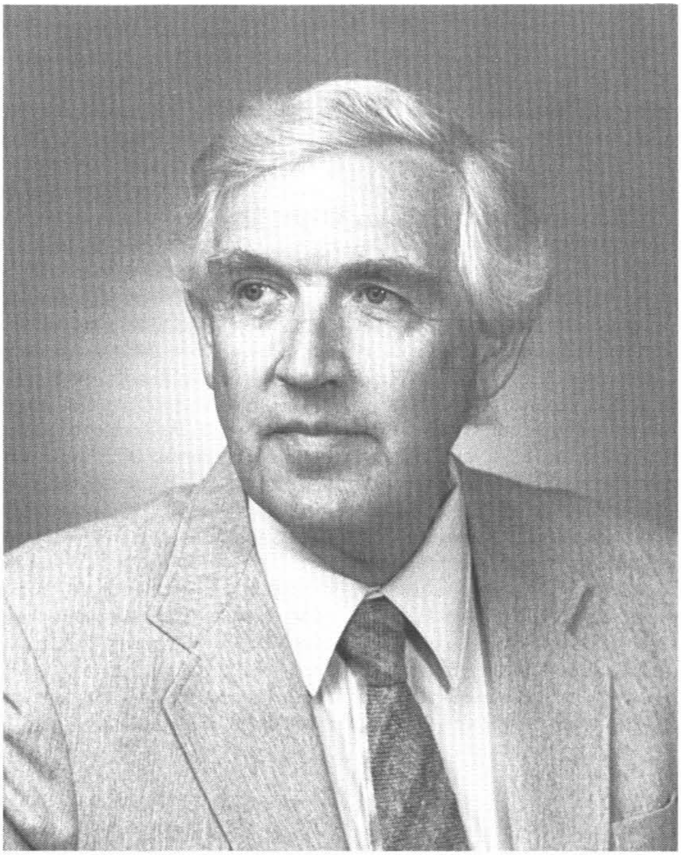

Professor R. F. Scott

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Failure

R. F. SCOTT*

'Failure: an ill-coined and late word'—Skeat,
An etymological dictionary of the English language, 1893

The Pennsylvania Disaster (1889 Johnstown Flood)
by William McGonagall, Scottish poet and tragedian

''Twas in the year of 1889, and in the month of June,
Ten thousand people met with a fearful doom,
By the busting of a dam in Pennsylvania State,
And were burned and drowned by the flood—Oh pity their fate!

The embankment of the dam was considered rather weak,
And by the swelled body of water the embankment did break,
And burst o'er the valley like a leaping river,
Which caused the spectators with fear to shiver . . .'

An attempt is made to classify geotechnical failures, and these are illustrated by several examples. The experiences involved in trying to obtain remotely the properties of the granular material on the moon and Mars are summarized. Descriptions are given of several landslides in southern California but which have aspects in common with other landslide events in other parts of the world. The failure of two dams is discussed briefly. The problem of fault rupture as a hazard in its own right, apart from the associated generation of strong ground motion, is treated from the points of view both of faulting mechanics and also the effect of the displacements on structures. Methods of analysis including the finite element and finite difference techniques are considered with respect to the examination of failures. Discussion of the constitutive relations used in such methods is given, especially regarding stable and unstable material behaviour. Since the propagation of slip or rupture surfaces is important in many of the examples, emphasis is given to the implementation of numerical approaches in which unstable material behaviour can be employed, and which can give rise to the generation of slip surfaces or zones. Examples are given of the results of a discrete element method and the use of dynamic relaxation with the finite difference technique applied to some typical problems such as embankments, slopes and punch indentation. Attention is finally given to the questions raised in the design of structures, and the analysis and use of failures in this regard.

KEYWORDS: **analysis; case history; failure; finite elements; landslides; site investigation; soil properties; stability; stress analysis.**

Plusieurs examples de ruptures géotechniques sont cités en vue de les classifier. Des expériences sont décrites dont le but était d'obtenir à distance les propriétés des matériaux pulvérulents sur la lune et sur Mars. Des descriptions sont données de plusieurs glissements de terrain qui ont eu lieu en Californie du Sud mais qui avaient des ressemblances avec d'autres survenues ailleurs dans le monde. La rupture de deux barrages est discutée brièvement. Le problème de la rupture par faille analysé comme risque proprement dit, indépendamment du resultat de fort mouvement du terrain, est traité au point de vue de la mécanique des failles et aussi de l'effet des déplacements sur les constructions. Les méthodes analytiques, y compris les techniques à éléments finis et à différences finies sont traitées en fonction de leur application à l'étude des ruptures. Les relations des constituants employées dans de telles méthodes sont discutées, surtout en ce qui concerne le comportement stable et instable des matériaux. Comme la propagation des surfaces de glissement ou de rupture est importante dans beaucoup des exemples décrits, on accentue l'emploi des méthodes numériques dans lesquelles on peut utiliser le comportement instable des matériaux menant à la génération de surfaces ou de zones de glissement. Des exemples sont présentés des résultats d'une méthode à éléments discrets et aussi de l'emploi de la relaxation dynamique à l'aide de la technique des differénces finies avec application à des problèmes typiques tels les remblais, les pentes et l'indentation au poinçon. Finalement des considérations sont données concernant l'analyse et l'emploi des ruptures en relation avec les constructions.

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INTRODUCTION

When the invitation to deliver the 27th Rankine Lecture was made, the subject of failure was very much in the public eye, so that it became almost an obvious topic for the lecture. A few months before, the Mexican earthquake of 19 September 1985 had caused much damage and many deaths and injuries, principally in Mexico City. That earthquake, as is usual with seismic disturbances, had at least two surprises: the low levels of acceleration in the near-source area along the Mexican coast and the high levels of acceleration in the valley of Mexico. On 13 November 1985 the eruption of the Nevado del Ruiz volcano in Colombia, South America, led to mudflows (lahars) which took many lives. Lastly, as far as immediate events were concerned at that time, in 1986 the failure of the Challenger booster engine and the loss of the spacecraft and crew left an entire space programme in disarray.

These events led me to consider the instances of failure in my professional life, since the incidents closely paralleled my interests at various times in my career. A few examples of failures are given which I used to learn something about deformational and failure processes in geotechnical engineering, and then I go on to ratiocinate about the role that failure and its analysis plays in the geotechnical field.

FAILURE CLASSIFICATION

Failures of geological and soil materials can be divided into a number of classes. The following tentative arrangement based on personal observation is suggested. There are three components of a failure; for it to be understood in the most general sense, information is needed on all three. They are *mechanism*, *properties* and *analysis*. We must have a clear picture of the *mechanics* involved in a failure before any subsequent steps can be taken. The initial emphasis of failure investigations is usually directed to the elucidation of the mechanism. Examples are the failure of dams, as described briefly later, or the collapse of a structure in, say, an earthquake. The sequence of events is important. Secondly lies the determination of both the qualitative and the quantitative nature of the material *properties* which rendered the failure possible. Was the soil easily erodible? Did it lend itself to hydraulic fracturing? Was it susceptible to creep? Was the stress–strain relation unstable? Then, subsequently, what were the peak and residual shearing strengths, modulus values etc.? When these two components have been classified, *analyses* can be attempted, incorporating mechanism and properties. Sometimes the examination of a failure only leads to the conclusion that it was caused by lack of application of already known principles. On other occasions a new mechanism or a consequence of material property variation becomes apparent, and then new precautions and analytical procedures are added to our lexicon.

Each failure can be classified in a broad sense by the extent to which the contribution of each of these components is understood. Here such a classification is illustrated by several examples, which refer directly to the brief case histories described subsequently. Other combinations of the three components may be readily inferred.

(*a*) An analysis can be performed based on measured material properties in standard tests, and which offers an acceptable explanation of the failure. When this condition pertains, a failure can be deliberately induced to evaluate the material properties. Here, mechanism, properties and analysis are all established.

(*b*) A mechanism is apparent, and an analysis can be performed, but its employment with material properties measured in standard tests does not give a result corresponding to observation.

(*c*) The cause or causes are minor defects in the material, or unclear mechanisms, which may be destroyed in the failure or which cannot be detected by investigation, are not amenable to analysis and can only be speculated about in post-failure investigations.

(*d*) The mechanism is clear, and a dependence on material properties is also relatively clear, but the material properties cannot be obtained for analysis, because of the nature of the material, economics, the dimension involved or the statistical variation of the material characteristics.

(*e*) For design purposes, a failure analysis is required but it is not obvious how it should be done since possible failure mechanisms are not clear. Prototype or model tests are called for.

The safety of existing or proposed structures is evaluated according to assumed mechanisms of failure and known properties, and therefore lies in class (*a*). If a possible failure mechanism requiring a lower load or stress than needed by the assumed mechanisms exists, it will operate and the structure is in trouble. It may eventuate that a failure occurs, which is entirely explicable, but only after more field or laboratory tests have been made. On subsequent study, the failure which occurs can fall into any of the other four categories. Category (*a*) constitutes the state of the art of failure analysis; new results come from failures which fall into the other groups. For those failures which lie in category (*b*), other processes, or mechanisms, have to be adduced to provide an explanation. On occasion, this has led to new results (Skempton, 1964) and altered analyses.

In many circumstances it is not possible to determine the detailed events leading to a failure, but design or construction methods can be proposed for future structures which will lessen the chances of a similar event. Peck (1981) has described such cases, which I place in category (*c*). Many failures involving difficult-to-sample dry or crumbling soils and fractured rocks may be placed in the realm of category (*d*) because the in-place behaviour cannot be related to the broken or partial samples obtained. Landslides involving cubic kilometres of material have occurred (Bolt, Horn, MacDonald, & Scott, 1975) and any reasonable assessment of material property, or perhaps even of original geometric configuration, is impossible to arrive at. There are conditions where the analysis or failure configuration is not obvious and small-scale or full-size tests are required to establish a failure mode to lead to an analytical model: these are referred to category (*e*). In some cases, legal ramifications are such that no involved party wishes to investigate the detailed nature of the failure.

The failures described in the following will be identified with these categories, wherever possible. Benjamin Baker's (1881) famous remark, 'if an engineer has not had some failures [with retaining walls], it is merely evidence that his practice has not been sufficiently extensive', is still applicable today.

FAILURES ON OTHER PLANETS

'It's all very pretty but I don't see that it proves anything'—comment by Senior Wrangler after reading 'Paradise Lost' (Crowe, 1967)

The moon

The first question of a serious nature concerning failure, and which affected my life, as it turned out, for many years, was directed to me from an engineer at the California Institute of Technology (CIT) Jet Propulsion Laboratory (JPL) in Pasadena. He said, 'How far would a sphere about 3 ft in diameter, weighing about 100 lb on earth and travelling about 100 ft/s penetrate into soil on the moon, if there were soil on the moon?'. The JPL began, and continues, as a laboratory of the CIT, which manages it for the US National Aeronautics and Space Administration (NASA). After the shock of the Soviet Union's first Sputnik, JPL had been given responsibility for planning the unmanned exploration of the solar system. At the time of the question (1959) planning was under way for the generally ill-fated series of spacecraft called 'Rangers'. That programme proceeded fitfully and frenetically, punctuated by regular failures, none of which, except for the final millisecond or two, involved any geomechanics.

At one stage it was intended to include a lunar impact capsule (Fig. 1) on the spacecraft; the capsule would be released and decelerated to fall, relatively softly, on to the lunar surface. The capsule included a seismometer to record tremors—moonquakes—on the moon. The development of the Ranger sphere gave the seismological and earthquake engineering communities the 'Ranger seismometer', which never recorded a moonquake, but which still provides excellent service as a sturdy but sensitive sensor in structural vibration studies. The JPL engineers were becoming knowledgeable about spacecraft design and performance by following Baker's precept but were lacking information on the mechanical properties of granular materials. Some studies of impact and penetration were made (Roddy, Rittenhouse & Scott, 1963), which required an estimation of a failure mechanism, and the properties of a dry sand at lunar gravity were estimated (Scott, 1964) to give some small assistance to the capsule design process. The penetration studies were of assistance later in the instrumentation of ocean floor coring and penetration devices (Scott, 1967a, 1970).

However, an intact sphere never reached the moon, since the first six spacecraft failed (Hall, 1977), causing a considerable diminution in the scientific squabble as to which scientific instruments should first be placed on the lunar surface. For experimental equipment, the last three spacecraft carried only cameras, with which to photograph the surface as the unretarded spacecraft approached it rapidly. A succession of pictures resulted, revealing the lunar surface at diminishing distances. The most remarkable feature was the similarity of the lunar surface at all scales, as each frame showed a lunar surface of lateral dimension decreasing from tens of kilometres to 30 m, containing only a random array of craters of assorted sizes. I do not know whether an intersecting mass of circles, similar at all viewing scales, can be described in terms of fractals (Mandelbrot, 1983), but I learned that, in much of science, increasing resolution does not bring with it increasing understanding; most funding agencies have yet to learn this.

The failures and delays in a programme whose planned 1962 termination was stretched out thereby to 1965 meant that the function of the missions had changed from one of scientific inquiry to one of support of the Apollo programme, announced by President Kennedy in 1961, long after initiation of the Ranger series. In the meantime my own extraterrestrial curiosity had been aroused by the interaction with the JPL

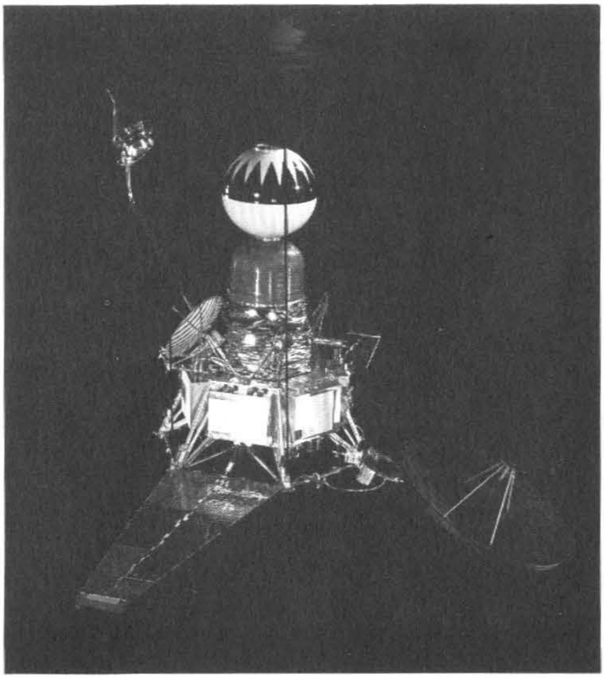


Fig. 1. Ranger spacecraft: the spherical hard landing capsule appears on top of the spacecraft (photograph by courtesy of JPL/NASA)

and, besides working on the studies already mentioned, I naturally wondered about the composition of the lunar surface. At the time there were two schools of thought

(*a*) the surface was mostly volcanic, and therefore likely to be relatively hard and rock like with volcanically generated craters

(*b*) the observed features were due to meteorite impacts, which would have generated a fair amount of granular debris of all sizes on the surface.

My natural inclination lay towards (*b*), and therefore in 1963 I proposed to NASA that a soil mechanics experiment be designed and flown on the next series of spacecraft being planned: Surveyor. The proposal was accepted, but it was a long time before the equipment went to the moon.

Since it had taken six failures to achieve the first Ranger success, plans were made for nine Surveyors; clearly the task of landing a survivable vehicle softly on the lunar surface was immensely more complex than simply sending a man-made meteorite to it. Of these, the first six were termed engineering vehicles, since their task was to test the feasibility of the concept. Science (which to NASA involves anything not pertinent to spacecraft functions and which includes what I would call engineering) was to be left to follow-on missions beginning with Surveyor 7; my surface experiment was identified with this definition. To indicate the difficulty of the task, I should point out that in the period 1963–65, there were five failures of Russian lunar soft landing spacecraft before their first success.

The Surveyor spacecraft (Fig. 2) had three legs equipped with non-linear gas pressurized shock absorbers and crushable pads; the final stages of the flight to the lunar surface were controlled by three so-called vernier rocket engines whose thrust and direction were under the command of a complex Doppler radar and inertia sensing system involving the spacecraft's attitude, range to the surface and velocity. These engines were to be shut down when the vehicle was a few feet above the lunar surface so that it would fall vertically under the gentle gravity of the moon, to strike the surface at about 3 m/s. Since no active tactile experiments were planned for the first six spacecraft, one of my students and I had calculated what would happen when the spacecraft hit a soil surface on a $g/6$ planet. With the infinite attendant combinations of three-dimensional spacecraft velocity components and surface slope and roughness, the computations were simplified to just the case of simultaneous contact of all three footpads at a range of velocities on a variety

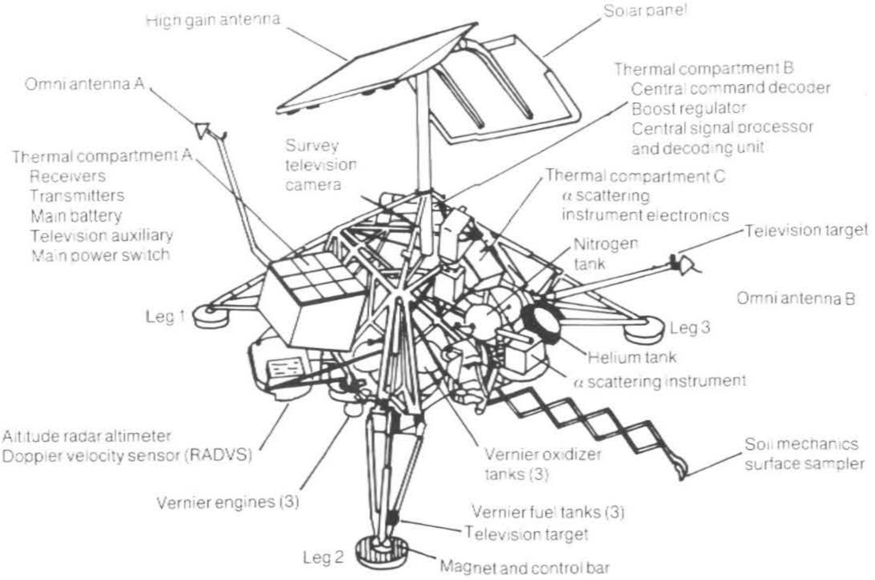


Fig. 2. Surveyor spacecraft (source: NASA)

of soils of varied mechanical properties ('soft', 'medium', 'hard' etc.) under lunar gravity and vacuum conditions. For engineering reasons involved with failure analysis of the spacecraft, the velocity at contact, the contact times on the three footpads and the force history in the shock absorbers were all to be measured and sent telemetrically back to earth. These data would only be of value to failure analyses if the impact occurred under something approaching the design assumptions—the associated ranges of the various parameters were known as the 'nominal' values.

Eventually, after many delays, the Atlas Centaur rocket combination containing Surveyor folded in its cocoon was launched on 30 May 1966. In view of the previous history of the lunar programme, no one was especially optimistic about the chances of success. The launch, however, and subsequent mid-course correction, went flawlessly. In the final stages of flight, to the small group in the Space Flight Operations Facility, the telemetry indicated an incredible degree of coincidence with the nominal mission. This has become a familiar (until Challenger) event, via the newspapers and evening television news broadcasts, but to that group at that time it was completely unexpected. The trajectory was excellent, the main retro-rocket fired and was discarded appropriately, the vernier engines started and operated as planned, and eventually the final, absolutely nominal stages of the descent were counted through to a perfect touchdown. The dazed euphoric group could only expect that the telemetry or camera would fail. They did not. A few minutes after touchdown, the velocity, contact times and shock absorber force histories were available. All three footpads had made contact with the lunar surface within 10 ms of each other, at a vertical impact velocity of 3·5 m/s. The computations for this had been prepared in graphical form on the first sheet of paper in my stack (case (*a*)), and a comparison established rather close bounds on the mechanical (soil) properties of the lunar surface material, within minutes of the first contact. Since the first picture had not yet been obtained, it was of considerable interest that, since the contact measurements and calculations demonstrated that the surface was neither rigid (rock) nor extremely soft, it probably consisted of granular material, although perhaps, to be broad minded, a porous, crunchy lava could not be excluded. The first picture, some time later (Fig. 3), of the nearest footpad confirmed the granular nature of the surface. The soil model obtained at the time has not changed in essence since. This and all spacecraft tests of planetary surfaces may be termed a category (*a*) failure.

In the early stages of Surveyor spacecraft planning, the design included a device for sampling the lunar surface material, so that the material could be supplied to chemical testing equipment. A preliminary model of this sampler was made by Hughes Aircraft Company in 1962; I tested it on soil and decided that, with some modifications and some instrumentation, it could serve as a useful device for soil property determinations. It formed the basis of my lunar surface experiment proposal. During the years up to the first flight, the modifications were incorporated in the sampler (Scott, 1967b), and extensive tests were conducted on a variety of possible lunar surface

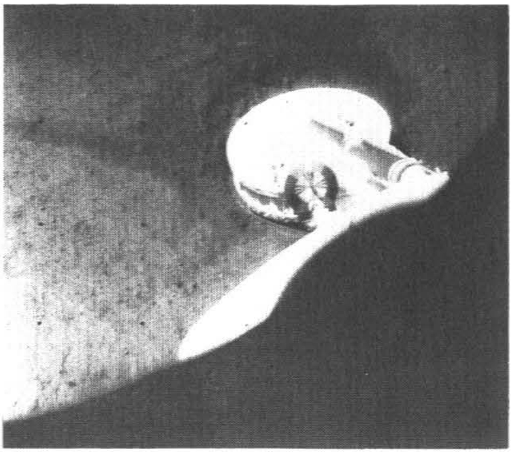


Fig. 3. Surveyor 1 footpad picture (photograph by courtesy of JPL/NASA)

materials from soils to rocks. The device could be extended, retracted, elevated, lowered and moved in azimuth by electric motors. An additional motor opened and closed the bucket door (see Fig. 2), enabling the sampler to pick up and drop lunar soil and rocks. It was possible to take pictures of the sampler at all points in its operational range from the Surveyor television camera.

The components of follow-on Surveyor spacecraft had already been manufactured and were in various stages of assembly, since subsequent launches were to be made every few months, at times dictated by the relative positions of the moon and earth. Surveyor 2 was complete and ready to go later in 1966 and was therefore not available for any modification at all; the schedule meant that the first vehicle capable of alteration was the third Surveyor, scheduled for an April 1967 launch. Efforts were directed to having the surface sampler accepted for flight on that spacecraft.

Because of the spacecraft's design, it was not possible to equip the surface sampler with force or displacement measuring instrumentation, but it was possible to estimate the force quite closely by using measurements of motor current, which was monitored. After some practice, displacements and positions could be estimated and subsequently measured from successive television pictures of the sampler.

In the latter part of 1966, the decision was made to fly the sampler on Surveyor 3 which, after final checks of the spacecraft, including the surface sampler, was launched on 17 April 1967. This flight proceeded without incident until the spacecraft touched down on the moon on 20 April. On command, the surface sampler deployed correctly.

My main objective with the surface sampler was to determine the mechanical properties of the lunar surface by failing it. To this end the tip of the surface sampler had been redesigned to present a flat area of about 1 in × 2 in to the lunar surface when the bucket was closed, and an area of about 0·1 in × 2 in when the door was wide open. The principal experiments available were bearing capacity/penetration tests caused by driving the bucket down into the surface at various extension distances. The maximum force available and the angle of contact with the surface varied with the distance. Trenching tests were also possible by pushing the open bucket down into the lunar surface and then retracting the sampler towards the spacecraft. A bearing test could be done in the bottom of such a trench, which could be deepened by successive passes of the sampler. Impact tests could be performed, and it was possible to pick up soil and small rocks to make an approximate estimate of weight and to relocate them.

From the tests performed, all of which involved failing the lunar surface material (Fig. 4), it was possible to calculate values of cohesion and friction for the lunar material and to estimate its density approximately. The soil was surprisingly, and to some extent, considering the effort involved, disappointingly normal in its behaviour and properties. The only somewhat unexpected feature was its slight cohesion, which did not appear to result from cementation but was persistent. In a press conference, trying to explain this feature to reporters, I described the soil as having the behaviour of a 'damp sand', hastening to make sure that they did not understand me to mean that it *was* damp, since there is no free water on the moon. A few years later, when soil was retrieved by the Apollo astronauts for study in terrestrial laboratories, it appeared that the cohesion was due to a random mosaic of positive and negative charges on the particles' surfaces, caused by solar wind bombardment. Adjacent particles therefore experienced attractive forces. From all points of view, the Surveyor 3 mission was a considerable success.

Surveyor 4 failed, but Surveyor 5 and Surveyor 6, without the surface sampler, reached the moon and performed well. The sampler was again installed on Surveyor 7, which, because of the delays in the programme, had been determined to be the last mission. That spacecraft, which also carried a deployable surface chemistry experiment, landed successfully near the large crater Tycho Brahe in January 1968. Apart from surface testing operations similar to those performed previously, the sampler was used to move the chemistry box about the lunar surface to test both undisturbed and reworked soil (Fig. 5). The

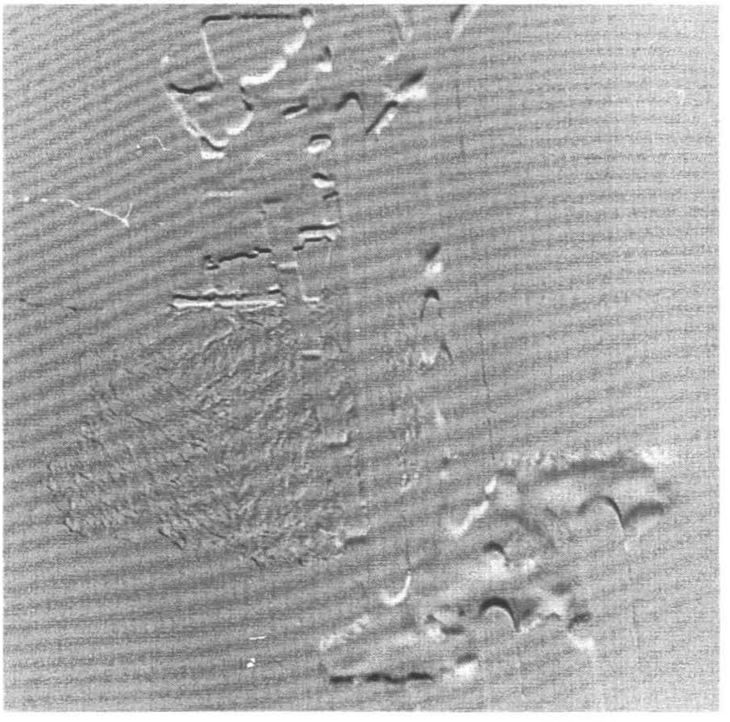


Fig. 4. Lunar surface test with sampler: two successive pictures in a penetration experiment were digitized and subtracted from each other to give this difference picture; this shows more clearly than either original picture the extent of surface disturbance, which can be used for property calculations (photograph by courtesy of JPL/NASA)

mechanical soil properties were similar to those determined before (Scott, 1967c, 1968; Scott & Roberson, 1968, 1969). With this mission, the Surveyor period was over.

By this time, I was becoming more heavily engaged in the Apollo programme as a consultant on lunar soil mechanics. However, I had one more remote sampling function to perform that year (1968). Plans were being initiated for an unmanned spacecraft to land on Mars in 1976, with a full complement of experiments. NASA's confidence had obviously increased. Requests for

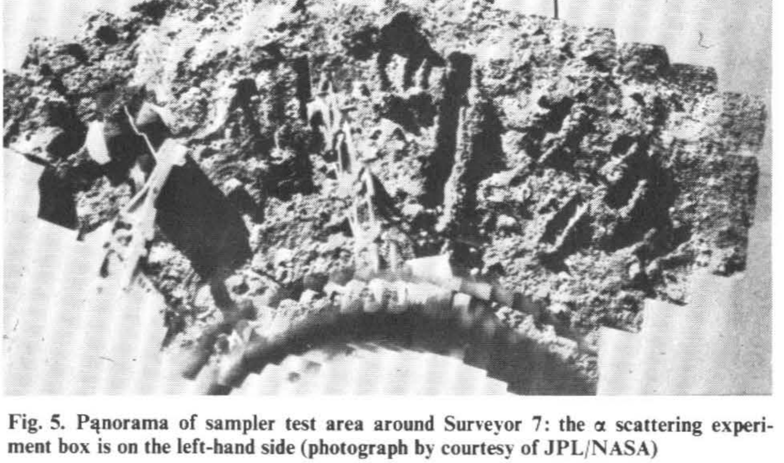


Fig. 5. Panorama of sampler test area around Surveyor 7: the α scattering experiment box is on the left-hand side (photograph by courtesy of JPL/NASA)

spacecraft design proposals had gone out to several US aerospace companies, including the Martin-Marietta Company (as it was then) in Denver, Colorado. Martin-Marietta hired me as a consultant to prepare preliminary designs of a spacecraft surface sampling tool for Mars, to perform essentially the same functions as on Surveyor, but with greater emphasis on obtaining samples of the Martian surface and delivering them to chemistry and biology experiments on board the spacecraft. Among other considerations, I suggested the use of a furlable tube. In the ensuing spacecraft design competition, Martin-Marietta was the successful bidder.

NASA records indicate that the design of the Apollo lunar module including landing gear had been finalized in 1964, to include, among other engineering details, footpads about 1 m in diameter. The engineering information provided by the Surveyor mission had not been used in the design. The lunar surface properties indicated that the lunar module's footpads only needed to have a diameter of about 1 ft—there would be no question of substantial penetration into any of the lunar surfaces that had been encountered.

Another area of interest was the tools to be employed by the astronauts on the lunar surface. They included sampling tubes, to be driven into the lunar soil with a hammer. These tools had been designed by the geological experiment team, none of whom had probably ever taken a *soil* sample. The astronauts had to be provided with a hammer, because geologists carry a hammer, in spite of the hazards of using it on the lunar surface, while the wielder was encased in a spacesuit, with a highly vulnerable visor. No other option was considered. A sampling tube had also been designed geologically, with a thick wall and a cutting edge flared on the *inside*, so that the tube's inner diameter was substantially smaller than that of the cutting edge itself. Since, in all the Surveyor experiments, the lunar soil had turned out to be dense, even remarkably so (Scott, 1968; Jaffe, 1973; Carrier, 1973), it was apparent that such a design, requiring strong shearing and deformation of the sampled material, with resulting soil volume expansion, would have little success on the moon. I prepared an alternative design of a thin-walled tube, with the usual soil mechanics feature of a constricted cutting edge, so that the soil could expand into a larger diameter after it had passed the opening, and some other features to facilitate sample handling. This design was not accepted. It was with some personal satisfaction, and a great deal of professional distress, that I subsequently watched, at Mission Control in Houston, as Armstrong and Aldrich on Apollo 11, and Conrad and Bean on Apollo 12, flailed away at their recalcitrant sampling tubes with little success. A new sampling tube (Fig. 6) appeared on the Apollo 15 mission, virtually identical in design with that of my proposal.

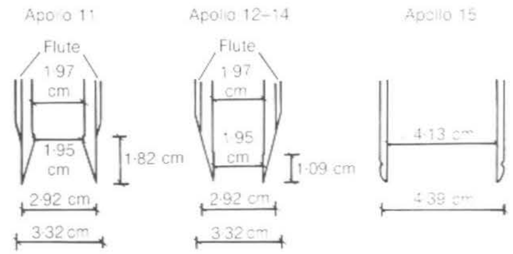


Fig. 6. Apollo sampling tubes: comparison of core tube bits (source: NASA)

However, before the flight of Apollo 11, my consulting arrangement with NASA had been terminated and I joined a soil mechanics experiment team, whose other members were W. D. Carrier III, N. C. Costes and J. K. Mitchell, which functioned in support of the Apollo group, participating in often acrimonious pre-flight meetings where scientists fought for their share of the astronauts' time on the lunar surface, as the schedule for the extravehicular activities of the crew was being assembled. Our tasks included analysis of the final stages of the lunar module's descent, the landing, sampling, coring and trenching activities, astronaut mobility on the lunar surface, soil mechanics aspects of other equipment placed on the surface and the performance of the wheeled lunar vehicle that was eventually used (Fig. 7). These activities have been well documented (Costes, Carrier, Mitchell & Scott, 1970; Scott, Carrier, Costes & Mitchell, 1971; Mitchell, Bromwell, Carrier, Costes & Scott, 1972) in reports and papers that will go unread until lunar bases are actively planned, if, indeed, they are employed even at that future time.

The astonishing success of Apollo 11 raised the immediate question of what to do next. At that time there was still a question of navigation on the moon; how close could the lunar module be brought down to selected co-ordinates? It was decided that an Apollo 12 landing near the location of Surveyor 3 would be an interesting test, while, at the same time, a visit to the 30 month old (April 1967 to Apollo 12 flight of November 1969) spacecraft might provide other useful information. That is where Apollo 12 landed in November 1969. Conrad and Bean, the astronauts, took some cable cutters with them as they pranced over to Surveyor, where they cut off the television camera and a portion of the surface sampler including the bucket (Fig. 8). When they returned, I was given charge of the returned

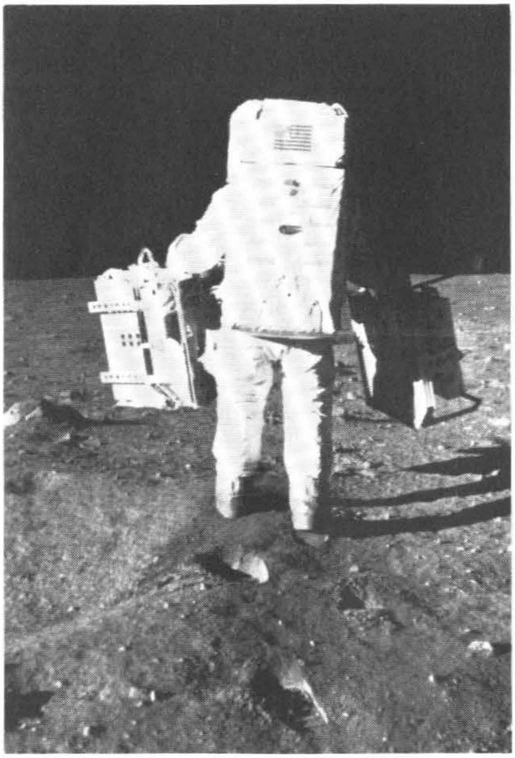


Fig. 7. Astronaut's footprints on the edge of a small crater: the penetration depth increases at the edge of the crater slope as the soil approaches failure (photograph by courtesy of NASA)

surface sampler for preliminary examination and photography, soil removal and cleaning. These operations were performed in a 'clean room' at Hughes Aircraft Company (the original makers of the surface sampler). I was able to do some simple experiments on the adhesion of the lunar soil to the painted surface of the bucket, which contained about 100 g of lunar soil that had been left in it at the end of the operation in 1967 (Scott & Zuckerman, 1971).

I had proposed some soil mechanics tests on the returned lunar soil to determine its properties in a relatively precise way, but standard soil experiments, such as the triaxial test, require 100 g or more of soil, and the Space Agency did not want to devote that much material to soil mechanics, even in a non-destructive test. However, after a time, the NASA Lunar Receiving Laboratory agreed to release some soil for soil mechanics studies—1·103 g. The vial containing the sample arrived while I was in England at Cambridge University in 1972 on sabbatical leave, and a miniature triaxial apparatus was designed since it is possible to place 0·8 g of soil into a triaxial cylinder 0·25 in (6·23 mm) in diameter and 0·5 in (12·7 mm) high. It was built in the Cambridge University Engineering Department machine shops (Fig. 9). One of the Department's undergraduates became interested in the project and performed some tests with me as a final year project (Boddam-Whetham, 1973). Since it was required to test the material with minimal disturbance to its natural state, the confining pressure was imposed by subjecting the specimen to a vacuum. Calibration tests were carried out on fine Leighton Buzzard sand, which was also tested both in standard 1·5 in dia. and larger 4 in dia. triaxial tests (Fig. 10). The apparatus and tests have been described (Scott, 1973) but detailed results have never been published. Some of the triaxial test data are shown in Figs 11 and 12. It

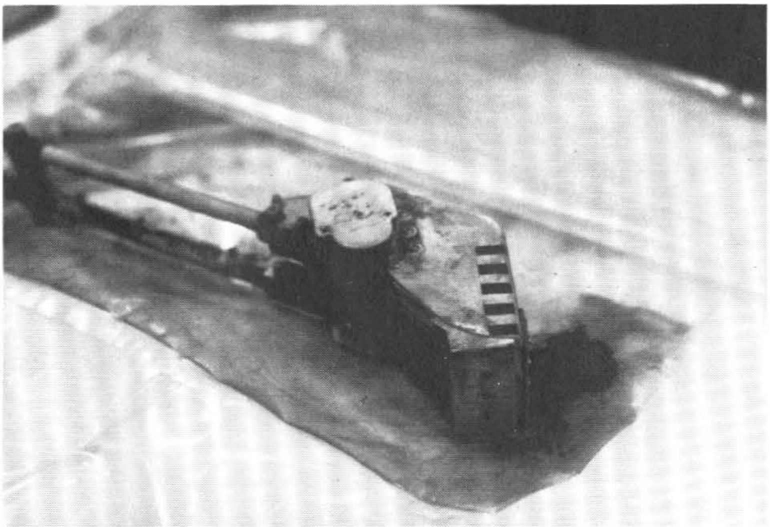


Fig. 8. Returned portion of the surface sampler

can be seen from these sand tests that there was little evidence of a scale effect on the *failure* behaviour of the material. It seems possible, from a shearing strength point of view, for sufficiently fine soil, to test all soil samples at 6 mm diameter taken, say, from 10 mm boreholes!

One of the interesting points to appear was the generation of slip planes in the dense samples at all test sizes. It has sometimes been postulated that a certain amount of shearing displacement rather than strain must take place before slip planes develop (Palmer & Rice, 1973). These tests in a miniature apparatus do not confirm this supposition, but indicate rather the usual appearances of slip surfaces, relatively independent of size. Centrifuge tests on model piles confirm this behaviour, since the axial load versus displacement behaviour scales well with prototype pile tests in terms of both peak load *and* displacement. Similar effects can also be observed with at least some slope tests. It can be seen in Fig 11 that the initial loading behaviour of the large specimens is much stiffer than that of the very small samples, at similar densities. The lunar soil has a higher shearing strength than the terrestrial sand, at the same void ratio.

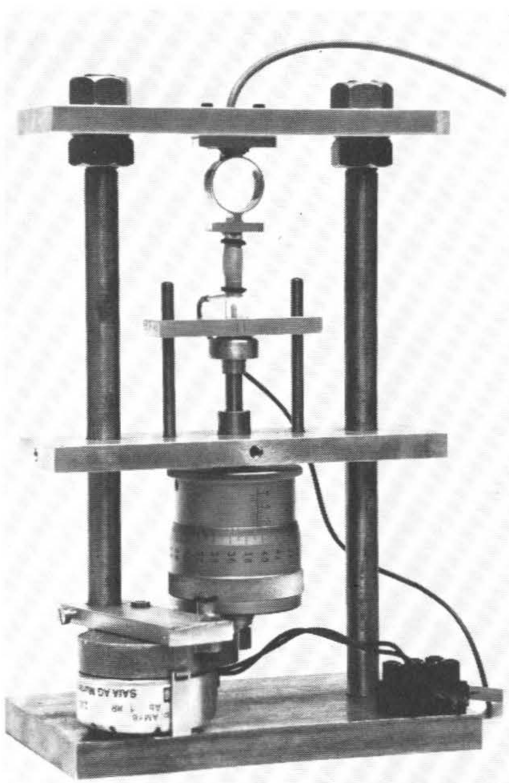


Fig. 9. Miniature triaxial test equipment: the micrometer is driven by friction from the electric motor at the bottom left-hand side

Mars

Since the earth, with its $1g$ of gravity acceleration, can be considered to be an ideal centrifuge in which to perform model tests of full-scale experiments on the $\frac{1}{6}g$ moon or $\frac{3}{8}g$ Mars, I had performed lunar and Martian model experiments in the laboratory at $\frac{1}{6}$ and $\frac{3}{8}$ linear scales respectively to study the geometry of soil failed by the sampling equipment. Those experiences and the Cambridge visit in 1972–73 stimulated my interest in centrifuge work. By the time of the 'sunset' of the lunar manned programme, plans were well under way for the Mars Viking missions. Properties of Mars soil were to be obtained, as on the moon, by failing the soil in bearing and trenching experiments performed by the surface sampler.

Before the flights to Mars, NASA had established a 'physical properties experiment' and team, consisting of R. W. Shorthill, R. E. Hutton, H. J. Moore, C. R. Spitzer and myself. The proposed experiments were described by Shorthill, Hutton, Moore & Scott (1972).

The first (of two) Viking spacecraft (Fig. 13) arrived at Mars on 19 June 1976 and landed successfully on the surface in the northern hemisphere at 22·3° N, 48° W, on 20 July 1976, after rejection of the pre-launch landing site, about 1000 km away, when high resolution Orbiter

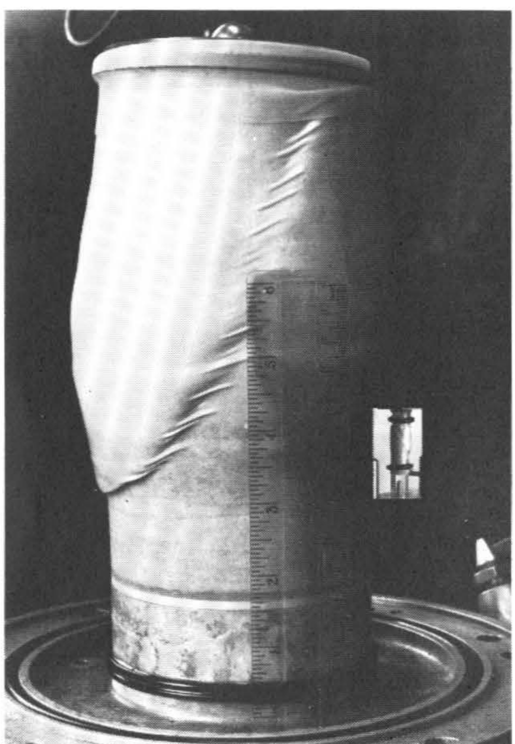


Fig. 10. Small and large samples after failure

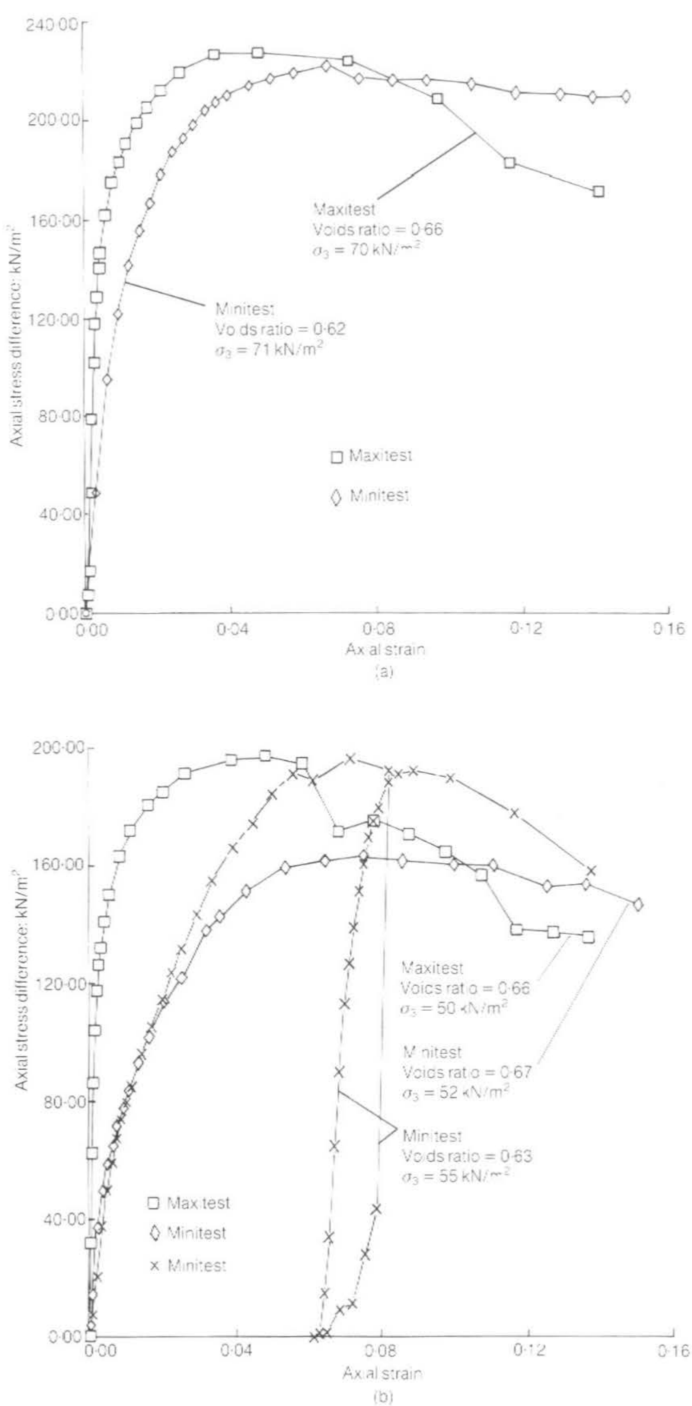
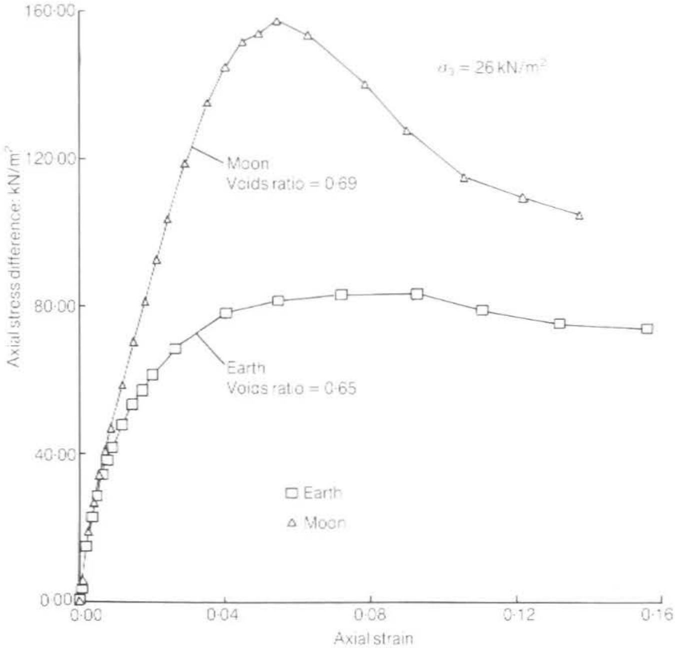
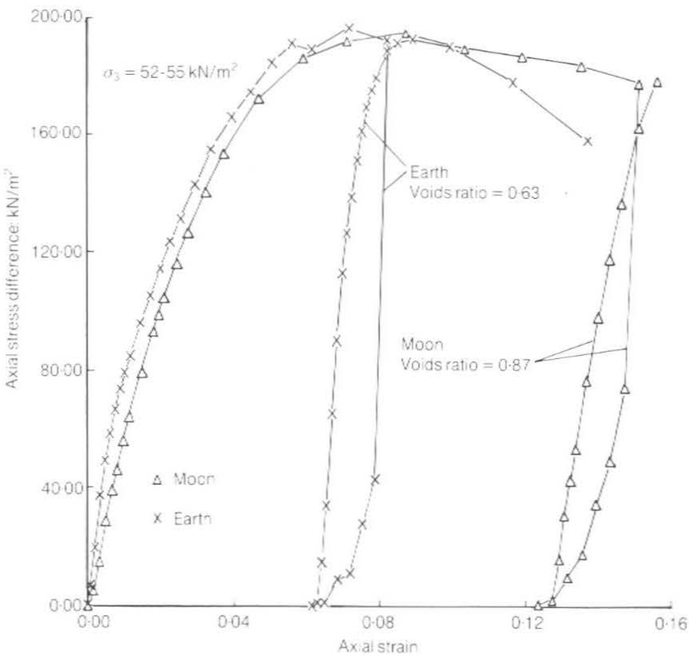


Fig. 11. Triaxial test results on Leighton Buzzard sand, for small samples compared with large samples: (a) confining pressure 70 kN/m²; (b) confining pressure 50 kN/m²



(a)



(b)

Fig. 12. Small test results on Leighton Buzzard sand and lunar soil: (a) confining pressure 26 kN/m², same void ratio; (b) confining pressure 52–55 kN/m², different void ratios

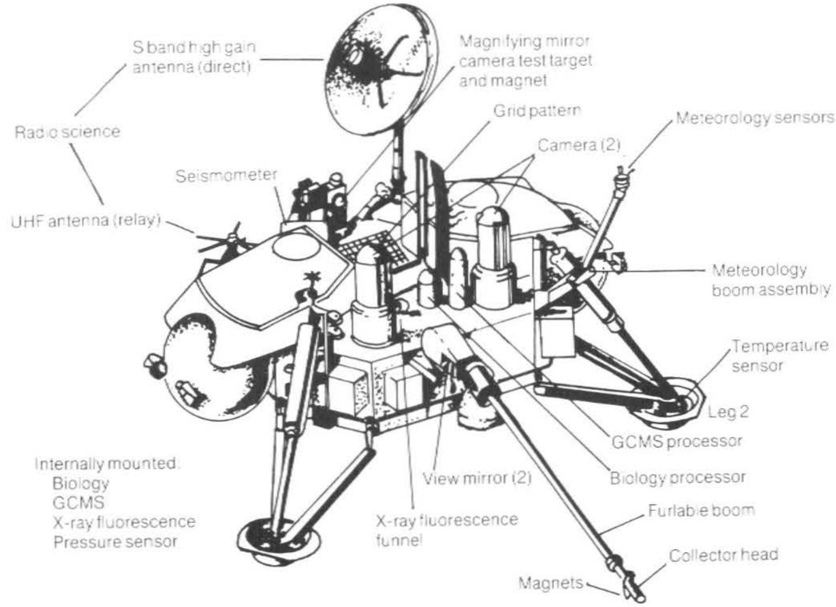


Fig. 13. Mars Viking spacecraft (source: JPL/NASA)

pictures of the surface were examined (Shorthill, Hutton, Moore, Scott & Spitzer, 1976). The biologists, seeking to maximize the possibility of finding life, had requested that the spacecraft be put down in a 'warm, low, wet' spot, all these terms being relative to Mars. The nature of the Martian surface had been less well known in advance than the moon's, but periodic Martian dust storms had disclosed the probable existence of granular material. The spacecraft possessed two imaging systems, operating on a different principle from the Surveyor's television camera, but providing an image (which could be in colour and stereo) by scanning the Martian terrain.

Early pictures showed a surface not, in general features, much different from that of the moon, but differing in a few atmospheric-related details. Little fillets of granular material were apparent behind individual rocks, and the tracks of small rock fragments moved by the descent engines' exhaust across the soil could be seen. Eventually the sampler went to work, dug trenches, performed bearing tests, moved rocks, sieved the soil into conical piles and obtained samples and supplied them to the other experiments (Fig. 14). The measurements of force and displacements enabled the Martian material's mechanical properties to be determined (Shorthill, Moore, Scott, Hutton, Liebes & Spitzer, 1976). Viking 2 landed six weeks later on the surface of Mars and performed a similar sequence of experiments (Moore, Hutton, Scott, Spitzer & Shorthill, 1977). Landers 1 and 2 transmitted data until November 1982 and April 1980 respectively. Although the experimental results revealed some anomalies, no unequivocal determination of the existence of life was made. The soil had the properties of loose to medium–dense fine sand.

LANDSLIDES AND DAMS

'All observation must be for or against some view, if it is to be of any service'—C. H. Darwin

At the same time as the Mars landers were working on the surface, cameras on the Orbiters were recording an astonishing variety of Martian landscape, including multiple landslides of sublime dimensions. If present conditions prevailed at the time of the slips, then they occurred in dry material with a very low gas pressure, yet many of the slides exhibit the character of flows. Is the gas pressure, low as it is, still sufficiently high to generate liquefaction during the sliding process, or was water present, or was gas released or generated during the shearing process by crushing of porous grains or by diffusion from physically adsorbed gas at particle contacts or in cracks in rocks? Similar, enormous slide/flow features have been observed on the moon (Fig. 15) in an almost complete absence of atmosphere. They are, of course, well known on earth (Fig. 16) where the mechanism of flow is still the subject of controversy. Shreve (1966) has suggested an air cushion below the flowing debris enabling it to travel great distances on flat slopes with minimal disturbance, although it is difficult to substantiate this mechanism with calculation. The relative run-out distance of such a slide seems to depend

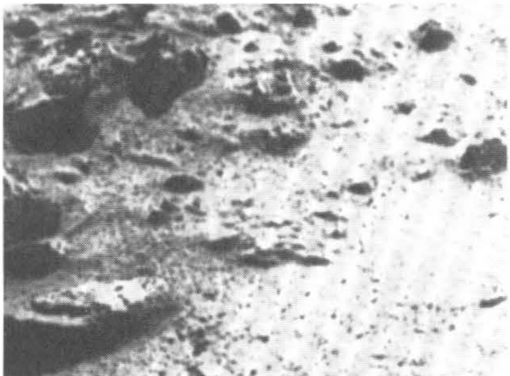


Fig. 14. Viking sampler test (photograph by courtesy of JPL/NASA)

on the mass of material involved, being greater for larger volumes.

Portuguese Bend

A somewhat different circumstance attends the Portuguese Bend landslide in the Palos Verdes peninsula, California (Jahns & Vonder Linden, 1973; Ehlig, 1982; Vonder Linden & Lindvall, 1982). It occurred in an area with a general slope of about 6·5° composed of the marine sedimentary rocks of the Monterey Formation of Miocene age, associated with volcanic intrusions. The Monterey Formation consists of siliceous mudstones, siltstones and shales, and includes bentonitic tuff layers and basalt intrusives of volcanic origin. It is underlain by Tertiary basalt and Mesozoic schist. Aerial photographs taken and geological studies before and after the Second World War, at a time when there had been little or no development, showed clearly that the region was a complex of large prehistoric landslides, of which the date of one was ascertained to be about 16 000 years Before Present (Vonder Linden & Lindvall, 1982). Ancient slide masses at higher elevations near the crest of the peninsula may have moved as long ago as 200 000 years. The slides may have been related to sea level changes and to tectonic uplift of the peninsula during glacial and interglacial episodes. In the early 1950s the area was subdivided and houses were built over a substantial portion of one of these ancient landslides. To provide access to them an extension of an existing road was begun by the County of Los Angeles down into the landslide area. In 1956 cracking of the road fill and breaking of water pipes was observed. A portion of the prehistoric landslide had been reactivated (Fig. 17). Investigation revealed a slide plane in a highly tuffaceous section of shale, in bentonitic clays at a depth of about 100 ft below the surface. Flow of the slide material destroyed or damaged most of the homes over an area of approximately 300 acres; of the original 156 dwellings, only 22 remained by 1982, all of which were supported by underpinnings, and, in some cases, jacks, which are periodically readjusted.

In spite of attempts at stabilization, the landslide, with a volume of about 40 million yard^3, has continued to move up to the present day (30 years), at rates of up to 20 ft/year, after the initially more rapid motion. In a period of unusually low rainfall in the 1970s, movement slowed down considerably, leading to hopes that the slide would stop, but a few wet years occurred at the end of that decade, and the movement rate increased again. The slide at present is displacing several feet a year.

Since 1956, movements of other portions of the ancient landslides have occurred, and some other areas are also displacing at the present time. There are also sections of the prehistoric landslides which are not currently developed, and the question arises at intervals about the possibility of construction being carried out safely in such areas. In the Portuguese Bend area, it seems clear that the clay of the bentonitic tuff has a residual friction angle equal to the average slope of the slide plane (Skempton, 1964), of about 6–7°. Since the last prehistoric slide, assuming that it occurred in the same material on about the same slope angle, the clay accumulated enough cohesion, probably by desiccation, with associated chemical changes and particle bonding (Bjerrum, 1967), to maintain stability of the slope under the gradual natural processes of erosion, and high and low rainfall periods, for, by geological inspection, at least a few hundred years (estimated on the basis of the appearance of some head scarps), and in some cases tens of thousands of years.

Rainfall has several effects on the slope, by

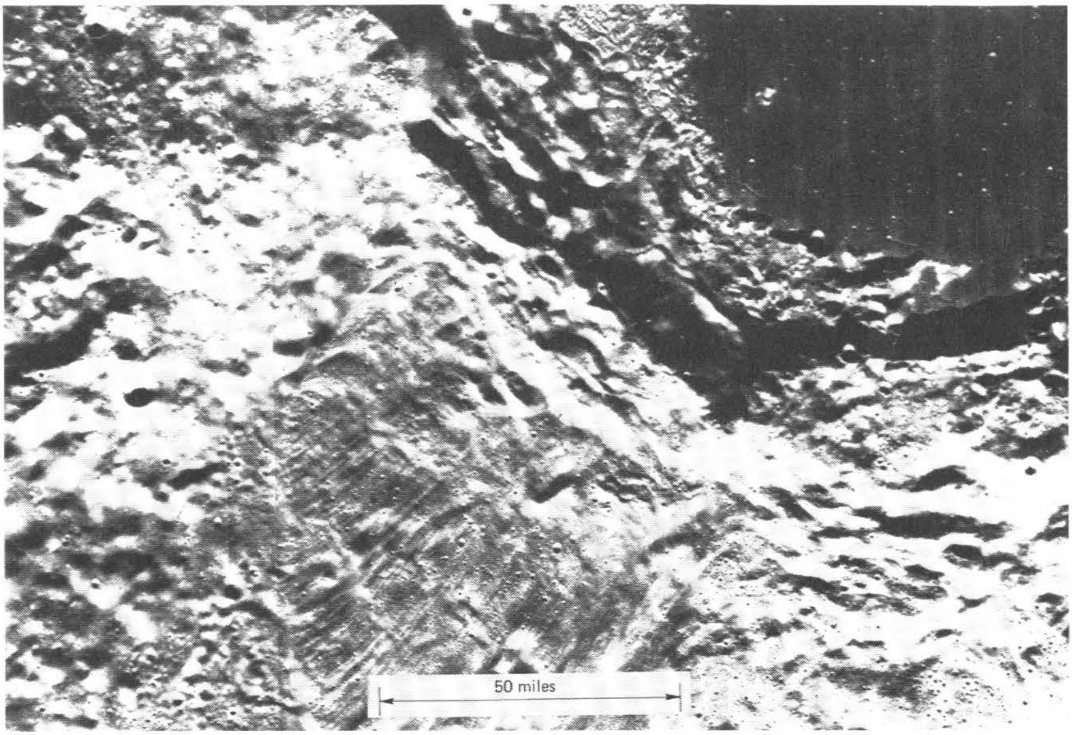


Fig. 15. Flow slide near Tsiolkovsky on the far side of the moon (photograph by courtesy of NASA)

simply increasing the weight of the overburden, by exerting lateral pressures where it fills cracks, by raising the water-table, and thus decreasing effective stresses, and by infiltrating the clay to alter its properties. Measurements of the rate of slope movement during periods of rainfall (Ehlig & Bean, 1982) have clearly indicated an increase in the rate of movement within a few hours of rainfall, showing the sensitivity of the stability to the influx of water into fissures, and to the increase in weight of the sliding mass, before any diffusion of the rain-water into the underlying clay could have occurred. Indeed, in intact areas outside landslide boundaries, the rise in water-table lags rainfall by several months (Ehlig & Bean, 1982).

What was the mechanism of the reactivation of the ancient landslide? Was it a change in acting load due to engineering activities and continuous erosion of the toe of the slide which emerges at the coast, or an alteration of the clay properties by infiltration of groundwater both from rainfall, which had occurred throughout geologic time, but now from household water use and with changed drainage patterns? If the process, as described by Bjerrum (1967), of a rupture surface propagating with time from the toe of the slope, took place, then it occurred without observation until the final breakthrough. The deformations accompanying such a rupture propagation are fairly small up to this point and are frequently noticed by residents, as mentioned again later, but are not generally interpreted correctly until the landslide develops fully. During the road construction referred to earlier, approximately 150 000 ton of fill were added to the north-east section of the ancient landslide. It was here that movements were first observed in August 1956. In subsequent litigation, the court ruled that the work on the road had triggered the landslide which spread from the north-east corner of the ancient event to cover a much larger area in subsequent months. Total damages assessed against the County of Los Angeles amounted to US $9·5 million. Although the cause has therefore been established legally, doubts remain about the real cause, particularly about the role that groundwater played in the failure mechanism.

How can the degree of stability of adjacent areas, whether underlain or not by ancient landslides, be established? The conventional soil engineering approach is to bore holes, to take samples, to perform soil mechanics laboratory tests, and to engage in slope stability analyses with and without the modifications to be imposed by the proposed new construction. However,

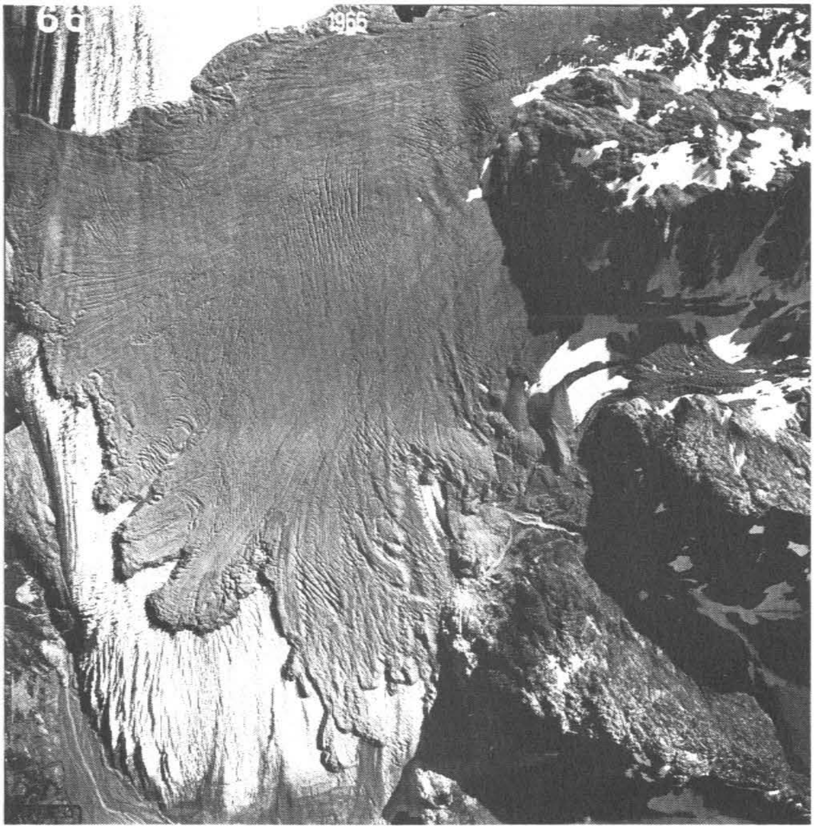


Fig. 16. Sherman Glacier flow slide, photographed $2\frac{1}{2}$ years after the Alaskan earthquake (photograph by courtesy of Austin Post, US Geological Survey)

there are zones of the bentonitic material at various depths, old slide surfaces can sometimes be recognized, and sometimes may not be, and the bentonitic tuff itself is difficult to sample, although cores can be obtained with care. In its natural state, it is apparently relatively dry, friable, much jointed and fractured, and after sampling it is almost impossible to prepare for a conventional, or even unconventional, shearing strength test. If, with much diligence and patience, a few specimens are prepared, the results are erratic as the samples rupture and shear along arbitrary cracks, at a wide range of strengths. The only feasible course is to remould the samples with water (what chemistry?), to consolidate or overconsolidate them and then to shear them in a ring shear apparatus or other equipment which allows sufficient displacement to give the residual shear strength. The analysis itself can be simple, since geometrically it has the form of an infinite slope failure, with possibly a breakout through a more competent layer to the free surface. Failures such as this may perhaps be placed in category (*d*) cited earlier.

At a potential development area, situated on sloping ground, with lateral dimensions of 1 km or more, by how much does the peak strength of the clay exceed its residual strength, or is the peak strength irrelevant? Is the residual friction angle of 6·5° (laboratory tests of pure sodium montmorillonite clays give residual friction angles of about these values (Olson, 1974)) indicated by the Portuguese Bend landslide uniform over the entire region? How will the strength be affected by future water influx to the region? When an area is developed in naturally semiarid southern California, the input of water into the ground increases markedly, from landscape irrigation and household use. In the Portuguese Bend area, no piped sewage system was included in the development (this is also true in adjacent developed areas which are also on ancient landslides); instead each house was supplied with a septic tank and a seepage pit. Household water was thus delivered directly into the ground and has been estimated at least to double the water quantity entering one of the landslides in the area (Ehlig & Bean, 1982). From the events that have occurred, the only safe approach is to assume that the site is clearly of marginal stability and must be stabilized by

Fig. 17. Portuguese Bend landslide, 1974: the head scarp is visible at the top of the photograph; side shear zones are indicated by road patches; the toe of the slide extends from the peninsula at the bottom left-hand side to where the housing begins at the bottom right-hand side (photograph by courtesy of Metrex Aerial Surveys Inc.)

strong measures (buttress fills in trenches of substantial depth) unless development is to be prohibited, except perhaps as a public or private park. Even in this case, irrigation would have to be eliminated or minimized, and it would be desirable to monitor water levels or pore pressures in the soil.

This is not a solitary opinion. A recent paper (Smelser, 1987) describes a 9 million yard^3 landslide, adjacent to the San Andreas fault south of San Francisco, that has moved in whole, or in part, since at least 1900. A tunnel, railroad and a highway through the area have successively been abandoned because of ground movements. The author of the paper cited concludes, 'The dynamic geology and the history of landsliding in the Mussel Rock area strongly indicate that the land is unsuited for permanent structures'. He suggests that it be used as a park for recreation and 'educational geologic field trips'. Since subtle movements may be occurring in ancient landslide areas, it would also be useful to conduct periodic precise surveys over them, preferably as part of a regional programme, but especially for several years in any area proposed for development.

Similar montmorillonitic clay zones are present in other locations in California and are usually identified with particular geological sequences, such as the Capistrano or Monterey formations. On occasion, the layer of volcanic ash laid down in an ancient volcanic event was perhaps only 1 mm or so thick and was subsequently covered by silt or sand deposits, which now make up weakly cemented siltstones or sandstones. With hydrothermal alteration, montimorillonite clay forms and is then present in these formations in laminae which may be 1 mm or less in thickness. It is accordingly difficult to identify in samples obtained from field boreholes. However, the thin layers have the shearing characteristics of montmorillonite and form weak zones among the siltstones and sandstones. Where tectonic processes have tilted the beds, which have later been eroded, hillside areas are found with the layers sloping at angles of a few degrees. Presumably on much steeper angles landslides removed the material in geological time. In the naturally arid state, and with sufficiently small angles, such slopes are relatively stable. However, if the slope angle is about 7–10°, stability is only present until some change occurs.

The process of destabilization of such a slope occurs by continuous erosion at the toe, by tectonic tilting, and the factor of safety lessens with both the weight and infiltration of rainfall. Human development inevitably introduces water of variable chemistry into the natural material, continually reducing the factor of safety, as it is taken up by montmorillonite layers. Under these conditions, it may not require an exceptionally wet winter to cause the additional erosion or increment of weight which incites the soil mass to slide on the fine clay layers, but this coincidence is often observed. When sliding takes place during or immediately after the wet season, it may be assumed that weight and possibly erosion, if the toe of the slope extends to a natural stream bed, are the direct causes of the slide. Frequently, slope failures occur several months or a year after the particularly wet season; in such circumstances, it must be expected that the slide was the result of additional water absorption by the clay layer after diffusion of the water through the overlying layers, or a delayed rise in the water-table.

Highland Park

One of these landslide events occurred in 1969, during a particularly wet winter, in Highland Park, Los Angeles. The movement occurred quite suddenly, with about 15 m of displacement in a few hours, and several houses were destroyed (Fig. 18). At the suggestion of a colleague, a student and I visited the slide a few hours after the major motion. The slide block had left a canyon at its upper end, and we climbed down into this chasm with a forlorn hope of identifying a slide surface. To this end one or two pits were dug in the debris at the toe of the uphill surface of the slide block. Nothing was particularly apparent, so during the next couple of hours we explored the remainder of the slide, marking a map with the cracks, shear zones and deformation observed. Before leaving, the test pits were visited again, and it was surprising and pleasing to find that a distinct fresh shear plane had developed, as the upper part of one pit was apparently attached to a still sliding block, whereas the lower part constituted unmoving bedrock. The movement was about 1 in/h. Some equipment and a recorder to measure the displacement continuously were put together and brought to the slide. A sample of the records obtained over the next few days is shown in Fig. 19. The slide was still moving, and slowing down, but not smoothly. It was moving in exponential increments, separated by astonishingly similar intervals of time. When these were apparent on the recorder plot, it could be stated within a fraction of a second when the next movement would occur. Disconnecting the recorder, exchanging gauges and using a different measurement system, all of which occasioned no effect on the movements, removed any doubt that the records obtained were due to the motion of the soil mass. The increments occurred on time over the period of equipment removal, substitution and replacement. During the subsequent month the movements became somewhat smaller in amplitude, but the slowing displacement mainly resulted from increasing intervals of time between individual motions. Another test hole, with instrumentation, located further uphill from the first, but still adjacent to the same soil block, gave results indicating that the block moved caterpillar fashion, with a movement propagating uphill at the block–bedrock interface, at a speed of about 0·3 m/s (Scott, 1978). The cumulative motion of the incremental record corresponded to the movements of survey monuments on the slide surface, indicating that the identified slide plane was, indeed, the only slip surface present. Samples of the siltstone rock including the zone on which slip occurred showed that the layer was montmorillonite a few tenths of a millimetre in thickness. Surveys showed that the slope was quite uniform, with an angle of 13°. In this case, the area had been populated with houses for many years.

Bluebird Canyon

In 1979, a landslide occurred in the Capistrano Formation at Bluebird Canyon in Laguna Beach, California. Many of the features were similar to previous landslide observations, but in this case the slide occurred in October, about six months after the last of the heavy winter rains, and again at the location of an ancient landslide. The new slide plane was a metre or two above the identified old plane. Once again, houses had been present at the site for many years, but the delay between rain and slippage implies that the slide may have resulted from changes to the sliding layer by diffusion or rising water-table rather than to the immediate increase in weight of the mass. Here the slope of the sliding surface was about 10°, but the surface was only a few hundred

Fig. 18. Head scarp of the Highland Park landslide, California, in oblique aerial view

feet long. The Bluebird Canyon slide was subsequently stabilized with compacted soil keys (Leighton and Associates, 1979).

In these slides, it would be of considerable interest to detect the early stages of the movement. When residents are questioned after a landslide, it is common to find instances of prior movement evidenced by cracks in driveways or structures, sticking doors and broken pipes in the ground. These may be observed as much as a year or two before the slide event. Generally, however, they are not communicated to engineers, or even among neighbours, and are usually attributed, in the young developed area of Los Angeles suburbs, to settlement of construction fills, and adjustments of the ground following construction excavation and fill operations, which also cause such disturbances. The problem of early detection is similar to that of recording the strong ground motion of earthquakes, which was such a difficult task 50 years ago. The location and time of the event are unpredictable, so that instrumentation must simply be set out and maintained for years, in the hope that it will be functioning when movement occurs. Self-contained acceleration recorders, which incorporate only inertial reference, were possible and were developed for earthquakes, whose economics justified deployment of instrumentation. For landslides, equipment is necessarily more complicated, and in some respects the problem is more difficult, since the movement is relative. For each station, two points are required, one on a potentially sliding mass and one on stable ground, and the distance between them must be continuously monitored. In a possibly unstable area, it is not easy to identify where these two points are to be located, it is difficult to devise a means for making the measurement in a place of public access, where extension wires, for example, will have a short lifetime, and continuous recording requires power, maintenance and record retrieval. Possibly, as for earthquake recorders, equipment could be devised to remain quiescent until relative displacement exceeded a predetermined value, but the problem of continual checking remains. When an earthquake occurs, the fact is immediately widely known, and the recorders can be examined expeditiously. The movement of slopes is more subtle, more localized and therefore requires repeated instrumentation inspection. A movement, in addition, may take place for days to perhaps years before a catastrophic slide occurs, if it does, and the sociological impact of data interpretation and warning, as is involved with earthquake prediction, is a significant question.

Baldwin Hills Reservoir

The main embankment of the Baldwin Hills Reservoir failed in December 1963; the failure has been written about extensively, most recently in a symposium at Purdue University in 1985 (Leonards, 1987). Briefly, the reservoir was constructed in an excavated natural valley in which the underlying material consisted of poorly cemented or uncemented siltstones and sandstones in a loose, friable state. In design (late 1940s) it was recognized that reservoir water

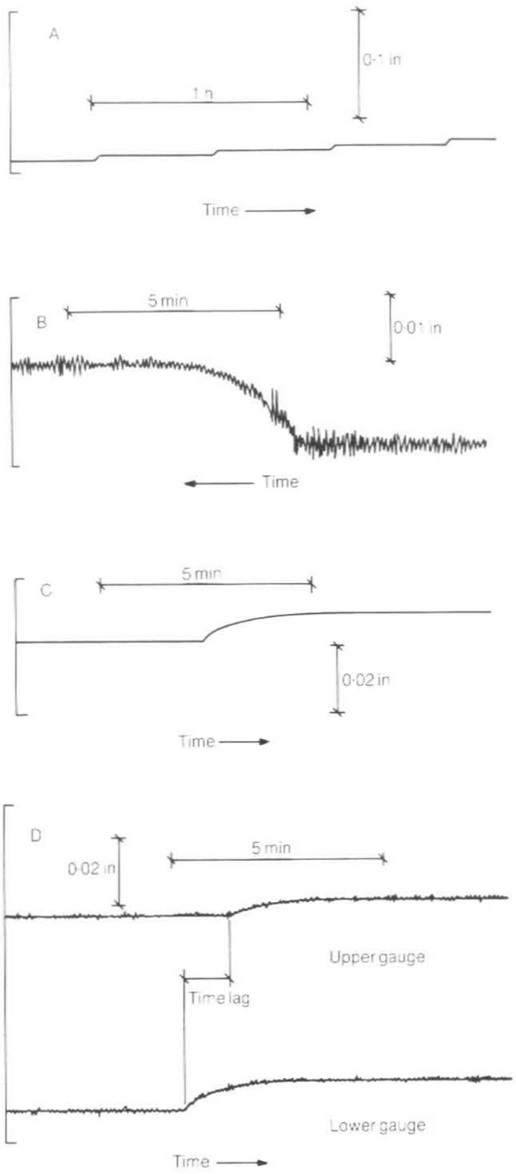


Fig. 19. Highland Park records: curve A, several events; curve B, one event, no filtering; curve C, one event, filtered; curve D one event at two separate stations

should be prevented from reaching those soils, so an impervious asphalt membrane was placed on the ground surface, a pea gravel drain system across the entire area of the reservoir built on top of it and the whole covered by a reservoir lining of 4–10 ft of compacted sandy clayey silt. It was expected that any leakage through the lining would be conducted by the drains to a drainage gallery below the reservoir, where it would be measured. Before and at the time of construction, the geological investigation disclosed that the site was intersected by several geological faults associated with the main Newport–Inglewood fault. In addition, it was recognized that the reservoir lay at the north-east edge of a subsidence zone caused by oil, water and gas withdrawal from the Inglewood oilfield. At the time of construction (1950) the maximum settlement at the reservoir due to subsidence was 0·4 m; at the time of failure it had increased to 0·6 m. In the design phase, engineering memoranda show that the designer and consultant board were concerned about the faults, but mostly from the point of view that the presence of the faults indicated the possibility of an earthquake, rather than with the consideration that one or more of the faults would rupture, displacing the material on each side and causing abrupt differential movements in the reservoir lining and embankments. In fairness, the idea that earthquake vibrations were caused by fault rupture and slippage was not so clearly understood at the time as it is now. Many earthquakes had occurred (and do occur) without fault slippage and differential displacement at the ground surface.

On completion of the structure, a complete programme of vertical and horizontal movement and crack surveys, drain leakage, water levels etc. was initiated. Data were recorded during the life of the reservoir. Early in its existence, the concrete drainage gallery which passed through two of the faults below the reservoir cracked, and cracked again. Brass plugs were established across the cracks and the measurements made on them were added to the data file. The cracks opened at an exponentially increasing rate up to failure. Flows into the drainage galleries varied erratically. Differential displacements and cracking at the embankment crest were observed along the line of the fault. However, no other obvious indications of trouble were observed until 14 December 1963, the day of the failure. That morning, the caretaker heard the sound of rushing water in the spillway which was enclosed in the dam, and whose inlet was well above the current reservoir water level. A short time later a vortex was observed in the reservoir near the main embankment, and water emerged from the downstream face at its contact with the natural ground, and along the trace of one of the faults. Futile attempts were made to plug the openings which had appeared in the upstream face of the embankment, but failure continued until a portion of the dam collapsed a few hours after the first observation of water flow.

The various failure investigations (Hudson & Scott, 1965; Casagrande, Wilson & Schwantes, 1972) concluded that the reservoir floor, including the drain system and the asphalt lining, had broken early in the reservoir's life along the lines of two of the faults, causing seepage through the lining to pass into the natural material along the faults and below the drainage system. For years water flowed into the fault zones and eroded large cavities in the soft material; presumably for much of the time this water, which was passing under the dam, was not in very large quantities, and so flowed undetected into the groundwater or into the unmonitored drainage system in the canyon downstream of the dam. It is surmised that, by the day of the failure, one of the cavities had enlarged sufficiently that the floor of the reservoir broke above it, releasing reservoir water into the chain of cavities along the fault. After this, total failure was inevitable.

The principal question which arises is what was the cause of fault displacement? It can be variously attributed to the following.

(*a*) Either a sudden geological movement within a day or two of the failure (this was ruled out, since an earthquake record study showed no relatable events) or a long-term creep of the fault akin to that observed on other faults occurred. This obviously would have to be occurring before and after the reservoir failure. There is evidence of fault movement near the reservoir before and after failure.

(*b*) There have been oil-related movements of the subsidence region which existed long before reservoir construction and continue to the present day. Oil, water and gas withdrawals and water repressurization have been taking place in the oilfield so that ground movements both down and up have occurred. The stresses and ground movements in the reservoir, which sat on the tension zone around the edge of the subsidence basin, were directly related to both withdrawal and injection procedures in the oilfield, but were the discontinuous fault movements caused by the pressure changes?

(*c*) A third possibility is the loads imposed by the reservoir itself. The load of the embankments is substantial, but the principal burden imposed on the underlying material is that of the 50 ft depth of water in the reservoir. The detailed post-failure investigations by means of trenches across the faults show signs of greater natural soil disturbance on the upper side (hanging wall) of the 70° dipping faults, although the evidence is not entirely convincing. It has been postulated (Casagrande *et al.*, 1972) therefore that the pressure of the reservoir water on the top of the reservoir lining caused greater consolidation and compaction on one side of each fault than on the other, which resulted eventually in rupture of the lining and drainage system as described.

What can be done to analyse such a failure? The measured horizontal ground displacements can be included, for example, in a large linearly elastic finite element model of the region of the reservoir along with estimated, but reasonable, values of the linearly elastic material properties. Two-dimensional models of this type were constructed as part of one of the investigations, including the possibility of tensile cracking (Casagrande *et al.*, 1972). The properties were checked by matching the observed rebound on reservoir unloading after the failure, but the initial stress state in the ground was not known. The calculations indicated that the oilfield-caused subsidence might induce a tensile zone to develop at the reservoir in spite of the reservoir water load. That is subsidiary evidence of a problem area, but not directly related to the failure. Since water undoubtedly, at some stage, broke through from the reservoir into the fault zones and may have accumulated there, an appropriate water pressure, to a guessed-at depth, can be included in such an analysis, and the results indicate lateral displacements and vertical movements due to the relief of shearing stress along the near-vertical fault surface. These movements can be compared also with the observed uplift in the reservoir vicinity, to confirm or refute ideas relating the uplift to repressurization programmes. However, any numerical analysis of this kind cannot contribute substantially to establishing the mechanism of failure of the Baldwin Hills failure. The undoubtedly non-linear material properties are not known in detail, and their time dependence may be important. Seepage, of the kind that was surmised, could not be included, and there was no mechanical model of the piping process. This leaves only speculative arguments about what went on. In such circumstances the analytical engineer is, and is likely to remain, helpless. However, the situation with respect to lessons learned for future design is not hopeless. There is clearly a good recognition of the basic causes of failure at this site. As recommendations, it is possible to say the following.

(*a*) Use extreme caution in a canyon or valley containing faults. Such sites are difficult to avoid in some regions of the world, such as California, where, to a large extent, the valleys are fault controlled. The site must be selected not only for the usual reasons, but to minimize the fault hazard. It is necessary to identify the faults, to date their movements if possible, and the amount and sense of those movements. The conclusions about such movements obtained from geological studies are usually largely qualitative and controversial. Could, on any basis, a fault movement of 8–9 in have been predicted for the Baldwin Hills Reservoir? When all the information is brought together, a defensive design may be possible.

(*b*) Do not build in a region of substantial subsidence. This recommendation is easier to implement, but may not be entirely avoidable. Numerical quantities of the magnitude and extent of subsidence are easier to obtain, however, and are more reliable than for faulting.

(*c*) The care which accompanies the design of drainage systems and controlled seepage zones in any dam must be reinforced to be sure that any displacements, abrupt or gradual, will not result in uncontrollable leakage.

(*d*) Monitoring systems are required that truly give information on what is happening in the structure in adequate time to carry out remedial measures in unforeseen consequences. If the event occurs (fault rupture, for example) on which design concern is centred, the monitoring arrangement must continue to function.

(*e*) The results of periodic surveys must be closely inspected and interpreted by engineering staff.

Teton Dam

The failure of the Teton Dam in Idaho in 1976 (Independent Panel, 1976; Leonards, 1987) was accompanied by similar intangibles. The post-failure investigation indicated conditions which left many engineers uncomfortable, without leading at the same time to definitive results that pin-pointed the exact mechanism of failure. Finite element analyses of the stress state around the key cut-off trench indicated stresses which raised concern about the possibility of hydraulic fracturing in the material as the reservoir level was being raised. However, discussion also centred on a 'wet zone' running entirely through the earth dam structure, and which was attributable to inadequate removal of fill which had been frozen during one winter of construction. As for the Baldwin Hills Reservoir, the Teton Dam failure will concentrate designers' attention on certain suspicious aspects of dam design, although the study of the failure itself has not led to a single conclusive mechanism. More analytical work, in the light of attendant uncertainties, is not warranted, and, indeed, it is not clear what conditions a possible analysis could simulate, nor what useful results would ensue. Both these engineering failures are placed in category (*c*).

GEOLOGICAL FAULTS, EARTHQUAKES AND CONSTRUCTION

'. . . not of practical importance to the engineer'—W. C. Unwin in 1911 Institution of Civil Engineers' Presidential Address concerning the efforts of 19th century elasticians

As discussed earlier, earthquake occurrence and slope stability events have much in common. In an earthquake, the accumulation of shearing stress due to mechanical processes which are only vaguely understood, on slip surfaces which have generally been ruptured before (an earthquake has never yet been observed to originate from a rupture in fresh rock), but whose properties are poorly defined, leads to a dynamic rupture event which radiates waves in a complex pattern. A landslide is caused by the development of shearing stresses, due to largely unknown changes in the causative mass (except in circumstances such as earth embankments, where material and geometry are relatively closely controlled), in soil or rock material which may or may not possess previous rupture zones, joints or cracks, to a point at which slipping occurs. Sliding may be a relatively static or a dynamic event. In both cases the mechanical causes are obscure, although less so in landslides, and the stress–displacement (constitutive) properties of the potential rupture zone, as well as the physical-chemical influences on it, are imperfectly understood. Since it is not possible to predict adequately the sliding hazard or safety of a specified natural slope, even when its geometry is well known and the possible sliding material has been sampled and tested, the frequent claims for funding for 'earthquake prediction', which has the same problems on a larger scale, in unknown materials which cannot be sampled, frequently offshore, at sites which cannot be precisely specified and are subject to unquantifiable stress fields, should be viewed with scepticism.

In site investigations for large structures, which, in the USA, used to include nuclear power plants, and still involves dams, oil and liquefied natural gas storage facilities, offshore oil structures and pipelines, faults are frequently identified during geological mapping. When they are, the question of their activity is foremost. Stringent guide-lines for this determination with regard to nuclear power plants have been issued by the US Nuclear Regulatory Commission. The difficulty of determining when a fault last displaced, or how many times it has moved in the last 10 000 or 10 million years is substantial. Investigations for nuclear power plants in the 1960s and 1970s generated much more quantified activity on the part of geologists, and trenching of alluvial materials became an important part of site investigations.

Once an answer has been obtained to the question of how often, how much and the probability of a movement in the 50 or 100 year life of the construction, the engineering task remains of designing the structure for the shaking to be caused by the causative fault rupture, or, on occasion, for the displacement which might be caused by rupture of a fault passing under the structure.

Propagation

In many parts of the world, including California, earthquake faulting originates in bedrock, overlain in some locations with many thousands of feet (as much as 20 000 ft (6 km) in the Imperial Valley) of alluvial soil materials. During an earthquake, the fault in the rock displaces as rupture propagates along it. It is surprising that this abrupt discontinuity in displacement continues on a slip surface running through kilometres of soil to the ground surface, where it appears as a sharp disruption of surface features. As mentioned earlier, the fault displacements observed to the present have occurred where previous faults are located, but since many faults (the San Andreas fault is an example) are characterized not by one break but by a zone of failure 1–2 km wide, new material must be, on occasion, disrupted. In the investigations in the early 1970s for a nuclear power plant in the San Joaquin valley of southern California, a fault was detected by seismic profiling means in alluvium many thousands of feet deep, near the site. The discontinuity did not extend all the way to the ground surface, but showed a gradually decreasing fault offset up to a level at which it could no longer be detected several hundred feet below the ground surface. One interpretation was that this was an old fault, on which activity had occurred many times as the alluvium was being deposited, so that the older alluvium evidenced all the displacement, but the youngest material recorded only the most recent movement. At some time, faulting activity ceased, but alluvial deposition continued, to give the upper few hundred feet of undisturbed deposits. The conclusion was that the fault was inactive. No historical earthquakes had been associated with the fault but the brevity of that record in California does not give useful information.

Another interpretation is that dislocation events might occur at bedrock of insufficient magnitude of displacement to rupture the soil all the way to the ground surface. A relative displacement across the fault would also be observed which diminished with the vertical distance from bedrock, but the implication of this interpretation is that the fault might be active, and shaking from a subsequent rupture event might be of importance to the power plant design. Depending on the amount, a further bedrock displacement or displacements might extend the rupture in the soil all the way to the ground surface. The interesting question was how much displacement would be required at bedrock to cause rupture just to reach the surface of soil of a specified depth? This problem could be placed in category (*d*).

Although faults have a variety of habits, two configurations contain all the features of particular engineering interest: a normal fault of vertical dip without lateral motion and a strike slip fault with no vertical movement. An analytical study of displacement in a material of fairly real soil properties increasing with depth did not seem to be achievable, so I began with a finite element analysis of the normal fault. At the time, failure could only be represented by a bilinear model incorporating a Mohr–Coulomb failure condition, and this was incorporated in the calculation. The results of several studies were presented (Scott & Schoustra, 1974) in terms of the amount of displacement required to develop 'failure' (by the criteria of the model) all the way to the ground surface or not, but the detailed results were only described in an unpublished report. In the calculations, failure was shown by shading those elements which reach the failure condition as the displacement is increased, as illustrated, for one example, in Fig. 20.

Since the bilinear model is stable, no slip surface appears, only a diffused yielding zone, which spreads upwards through the soil from the bedrock discontinuity. In the real soil, the rupture surface presumably develops because of an unstable stress–strain relation, but this could not be represented in the finite element computation. It was thought, however, that the stable model would give a conservative (too high) estimate of the required bedrock displacement (which it probably did). A disturbing feature of these analyses of the fault problem was that the failure region did not propagate in the correct way. If a basement region is uplifted as shown in the study, a slip line should appear, diverging from the uplift block to the left-hand side with reference to Fig. 20, as demonstrated in both model tests of anchors and slip line analyses. The numerical results demonstrated a yielding region propagating to the right-hand side. Would this be changed by the incorporation of a more complex and descriptive constitutive model for the soil in the analysis? In general, this is not known, because such models are not yet widely available in finite element codes. However, an analysis by a finite difference method, incorporating a non-linear model with a yield surface (Roth, Scott & Austin, 1981) was performed in association with the centrifuge faulting study illustrated in Fig. 21 and

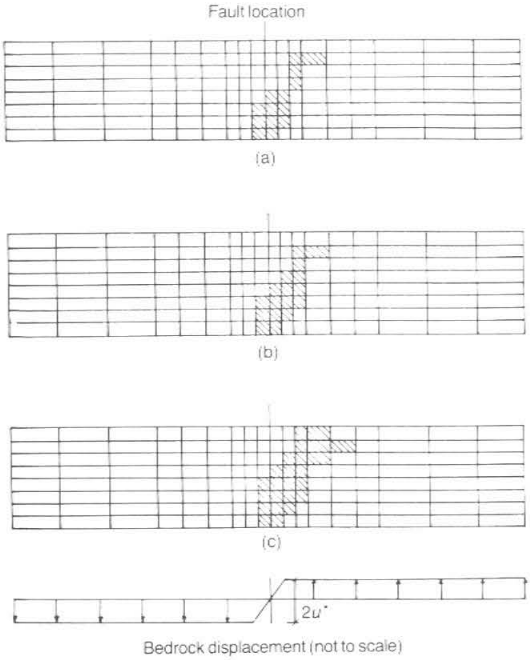


Fig. 20. Bilinear finite element solution for a vertical fault displacement of successively larger amounts (the yielded region is represented by the shaded elements; depth of section shown, 800 m; width shown, 4500 m, half of the finite element model; unit weight, 16·7 kN/m^3; cohesion 49 kN/m^2; friction angle, 25°; *E* varies linearly with depth to a value of 157 MN/m^2 at the base; Poisson's ratio, 0·4): (a) $u^* = 20$ m; (b) $u^* = 22$ m; (c) $u^* = 25$ m

produced the results shown, in which the calculations exhibit a reasonable match to the experimental results. More will be said about finite element and finite difference approaches to yield and failure problems later. The centrifuge study (category (*e*)) was performed to give more information on the question of the effect of an isolating zone of soil between a faulted bedrock and a liquefied natural gas tank to be placed on the soil surface and how an analysis might be performed.

Los Angeles Subway

The same subject matter, faults, has an impact on every kind of engineering venture in seismically active areas. It was decided several years ago that Los Angeles should have a subway to bring it into line with other major cities. Preliminary route studies and soil and geological investigations were initiated. It is not possible in the city to find any line joining suburbs to working areas which does not traverse one or more faults, some of which are considered active. All subway structures in the region must be designed for the

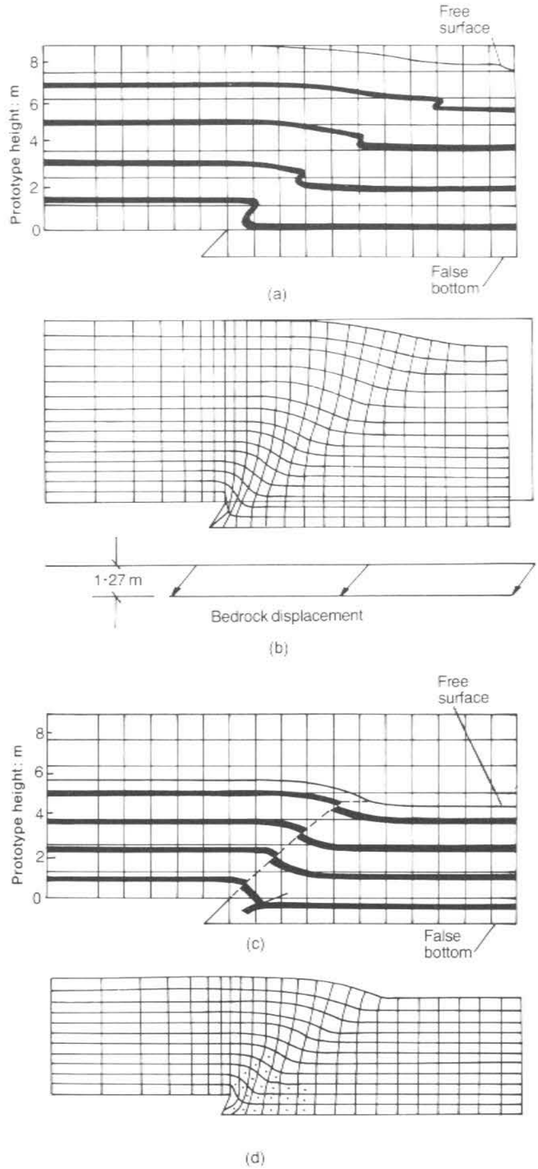


Fig. 21. Dynamic faulting through loose sand in a centrifuge: (a) centrifuge test result, loose sand; (b) finite difference analysis; (c) centrifuge result, remoulded site material; (d) finite difference analysis (the dots show elements indicating tension) (after Roth *et al.*, 1981)

expected level of shaking produced by an earthquake (the two usually employed events are the 'maximum expectable' and the 'maximum credible'; interestingly for potentially the largest earthquake-producing fault, the San Andreas fault in southern California, the two terms coalesce), but the crossing of an active fault by a subway tunnel introduces different design considerations. In consonance with the design philos-

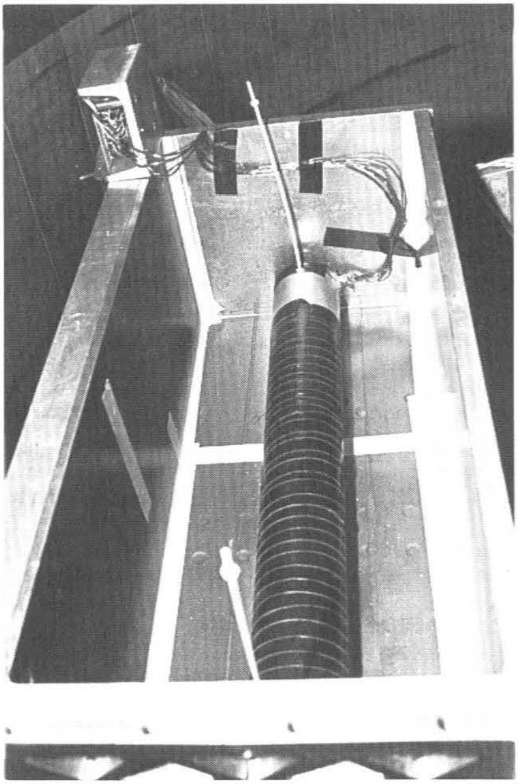


Fig. 22. Los Angeles Rapid Transit District centrifuge tunnel model (after Lindvall, Richter and Associates (1984))

ophy employed for above-ground structures, the subway tunnel should be expected to survive a large displacement without danger to the travelling public, although subsequent repairs and maintenance inevitably would be required. In other words, after the largest possible rupture on a fault through which the subway passes, the tunnel should remain open and accessible, even though vehicular movement may not be possible.

There are several subsidiary questions associated with this engineering problem. Those concerned with the probability of rupture at a fault, the magnitude of relevant relative displacement and the location of this displacement in the fault zone are not of concern here. The particular engineering problem is, given a specific displacement amount, what is the effect on the tunnel section and what special design might be required to maintain the tunnel open after displacement? It is assumed that the soil properties and tunnel section have been identified. The mechanics of this problem are again difficult to address analytically, it therefore falls in category (*e*), and some model experiments in the centrifuge were proposed, with the same test equipment as employed in the faulting study described before. The proposed tunnel was to be composed of precast concrete segments, bolted together, but it was not practical, for a preliminary study, to build a 1 : 50 scale model closely simulating this anisotropic real structure. Instead an aluminium tube was chosen, whose cross-section reasonably scaled the stiffnesses EI of the tunnel in both longitudinal and ring directions, and which could be easily instrumented with strain gauges and displacement transducers. The most worrying fault is the so-called Hollywood fault, a normal fault of 45° dip; the tunnel intersects it at 90° to its strike. The apparatus (Fig. 22) was already arranged to simulate a 45° dipping fault, so that no substantial alterations were required. The model tunnel was placed on 'bedrock', surrounded with soil compacted in different tests to densities spanning the field values, the centrifuge brought up to scale speed and the fault actuated. Some of the stresses recorded on the aluminium tunnel are shown in Fig. 23 for a fault displacement designed so that the aluminium would not yield.

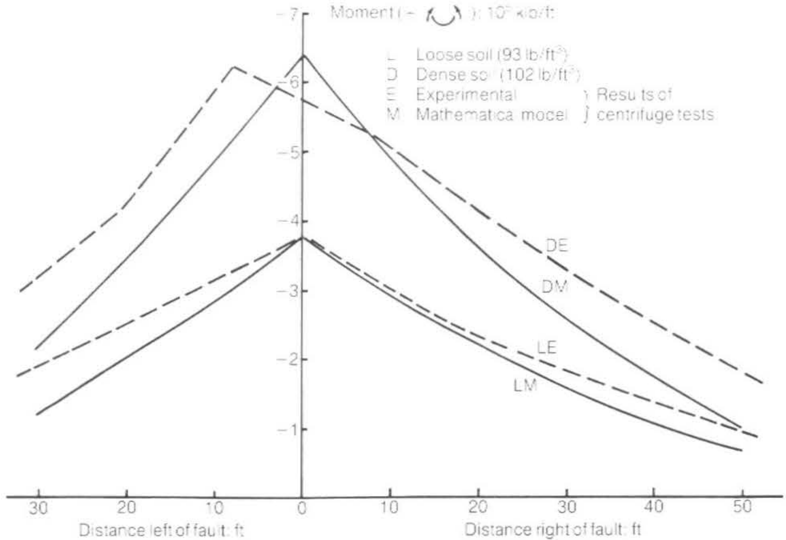


Fig. 23. Tunnel moments developed by fault displacement (after Lindvall, Richter and Associates (1984))

A complete finite element simulation of the model condition including yielding soil was considered briefly and rejected partly on grounds of economics, but mostly because of doubts that the soil behaviour could be properly (or even reasonably) represented. Instead a much simpler Winkler foundation type of representation was devised as shown in Fig. 24. The non-linear displacement elements were calibrated by matching the numerical model to the centrifuge test results with the boundary conditions imposed on the aluminium tube. The tube was finite in the tests whereas the subway tunnel will be effectively infinite in length. The stresses imposed on the tunnel under the condition of infinite length were obtained from the numerical model by maintaining the element constants derived by the test comparison, but increasing the number of elements to distances on each side of the fault at which negligible effects on the tunnel were observed. In additional studies with the numerical model, the fault displacements were also increased. For the estimated fault displacement required for the tunnel design, the stresses determined were greatly in excess of the strength of the tunnel segments. A different section of subway passing through this fault zone is therefore called for, possibly in the form of a steel tube, to maintain the integrity of the opening (Lindvall, Richter and Associates, 1984).

Discontinuous structures

'There is a limit to what we can do with numbers, as there is to what we can do without them'—N. Georgescu-Roegen

The engineering situations discussed so far have all fallen into an area in which continuum mechanics solutions are broadly applicable, although discontinuous results in the form of slip

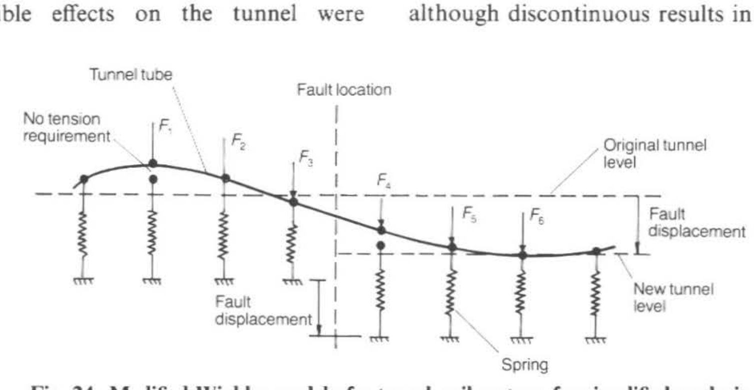


Fig. 24. Modified Winkler model of a tunnel–soil system, for simplified analysis (F_1–F_6 are overburden soil forces) (after Lindvall, Richter and Associates (1984))

surfaces emerge in the material behaviour. In much of rock mechanics, however, the principal characteristic of the material is not its continuous nature but its discontinuities. Individual blocks of intact material make contact with one another along fractures, fissures and cracks. In the response of a mass of such material to applied loads the properties of the intact blocks are of relatively less importance than the properties of contacts along the fracture surfaces. At the boundaries between soil and rock mechanics, problems involving this qualitative difference in behaviour arise where soft rocks are concerned, or possibly heavily overconsolidated and fissured clays. It is unfair, in these circumstances, to expect the application of a continuum mechanics method to give results reasonably representing the response of the jointed real material.

A difficulty of this nature arose a few years ago when it became necessary to analyse the seismic response of a ridge joining two separate concrete dams, Juncal Dam, in a water project in California (Fig. 25). The ridge itself formed a dam for the reservoir, in a roughly earth dam configuration, but it consisted of highly fractured and jointed soft rock. Following the 1971 San Fernando earthquake and the near failure of the Lower San Fernando Dam, questions were raised by the State of California Division of Safety of Dams (DSOD) regarding the stability of all major dams in California. The DSOD required all substantial dams to be inspected and analysed; depending on the consequences a dam might be left alone, rehabilitated, operated under reduced reservoir elevation or taken out of service. An order of priority based on the consequences of failure was established. In due course, the Juncal Dam system came to be examined to assess its seismic safety. The usual geological and soils investigation led to the identification of faults which could cause earthquakes relevant to the stability of the dam system, and the assessment of earthquake magnitudes and strong ground motion which could be developed by these 'design' earthquakes (Lindvall, Richter and Associates, 1982). The safety of the two concrete dams at these levels of shaking could be assessed by methods which had been presented or developed during the integrity

Fig. 25. Juncal Dams and Jameson Reservoir, California (after Lindvall, Richter and Associates (1982))

analysis programme, and which had been accepted by the DSOD, but the fractured rock ridge posed a different problem.

Earlier conventional 'pseudodynamic' slope stability calculations employing horizontal and vertical static accelerations to represent an earthquake's effect on the structure had raised questions about the stability of the ridge, but the assumption involved in these analyses required a relatively continuous slide surface through the jointed and fissured rock mass. No such continuous through-going fracture could be identified (Fig. 26) in the soil and geological investigation, and another approach was sought for an improved, and more realistic, analysis. The finite element method was considered, but it was not clear how the relevant material properties could be obtained; because of the fractured state of the rock mass, intact cores were difficult to obtain, and their properties were not pertinent in any event. Some large plate loading tests were performed in the walls of trenches near the crest of the ridge, to give information on the integrated response of the fractured mass, with a wide scatter of results. It was additionally not clear how the behaviour of the material in the core of the ridge would correspond to that of the near-surface material tested. Other testing expedients were considered. Finally, even if a finite element model were constructed and many analyses run to span a plausible range of continuum material properties, including non-linear behaviour, experience has shown that failure mechanisms are not displayed, but rather plastic deformational patterns emerge which are consistent with the continuum formulation. It is possible to formulate finite element geometry incorporating cracks, but the implementation of such a program for a dynamic input which would cause crack opening, closing and sliding was seen to be a formidable task, involving considerable development effort. A discrete element approach was more logical. Here the individual rock blocks between fractures would be modelled, with an appropriate fracture geometry. The material properties efforts would be directed towards a determination of the conditions at the block interfaces. Perhaps this effort would fall in category (*d*).

On request, Dr P. Cundall (now of the University of Minnesota) made his discrete element program, UDEC2, available, and the code, which was formulated for static problems, was modified to handle dynamic seismic conditions. In its original form the code functions according to a finite difference procedure along dynamic relaxation (Otter, Cassell & Hobbs, 1966) lines, so that, for the static problem, the forces in the system acting on each block at one time are used to calculate pseudoacceleration, velocities and displacements of the block, and from these the forces are recalculated for the next time step. After a time the velocities die down and the static solution emerges. The procedure has been widely used in the study of particulate behaviour (Cundall, 1976; Cundall & Strack, 1979; Walton, 1981) and will be referred to again later. After modification to handle a dynamic situation, the UDEC code was employed in a seismic analysis of the Juncal ridge in the configuration shown in Fig. 27 under the required earthquake acceleration versus time horizontal and vertical base inputs. The reservoir water pressure was included in the calculations. A larger number of blocks was desirable and would now be computationally feasible. As with the finite element computational procedure, a potentially bewildering amount of information on each

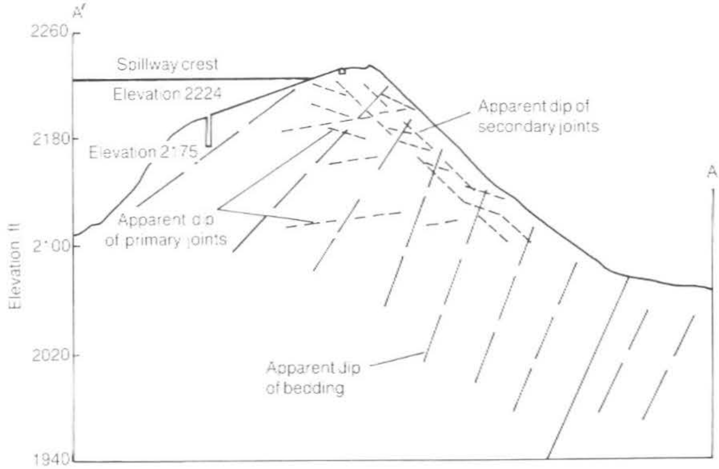


Fig. 26. Observed and inferred fracture pattern in Juncal ridge: cross-section through the auxiliary ridge (after Lindvall, Richter and Associates (1982))

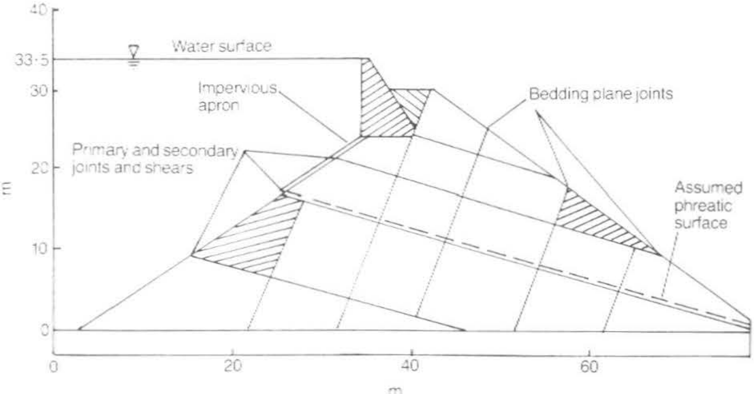


Fig. 27. Discrete element model of Juncal ridge (after Lindvall, Richter and Associates (1982))

block is available from the solution—accelerations, velocities and displacements in two components, as well as the same rotational quantities. A history of the making and breaking of contacts between the blocks can also be accessed. If a failure mechanism wishes to develop, the computation permits it to do so. If a complete and obvious failure does not occur, particular engineering items of interest are the crest displacement history and final displacements and the crest acceleration history.

Engineering decisions on the safety of the structure and operation of the reservoir must be based on these quantities, and these judgements are perhaps the most difficult aspects of the entire process. In the solution for the Juncal ridge various properties for the ridge substance were assumed for various trial analyses; one of the results felt to be representative of real behaviour is shown in Fig. 28. No definite failure mechanism emerged, but a lateral crest displacement of 1·5 m is evident; the vertical movement is almost as large. Since the ridge is 30 m high, this represents a movement of about 5% of the height. What is an acceptable vertical or horizontal displacement of an earth dam or an earth-dam-like structure during a strong earthquake? Santa Felicia Dam (80 m high) experienced only small vertical and lateral displacements during the 1971 San Fernando earthquake at crest accelerations of 0·2g with no apparent distress or hazard (Abdel-Ghaffar & Scott, 1979). In Mexico, in September 1985, La Villita Dam (60 m high) displaced 0·3 m vertically (0·5% of its height) with peak crest accelerations measured at 0·69g, whereas El Infiernillo Dam (140 m high) in the same earth-

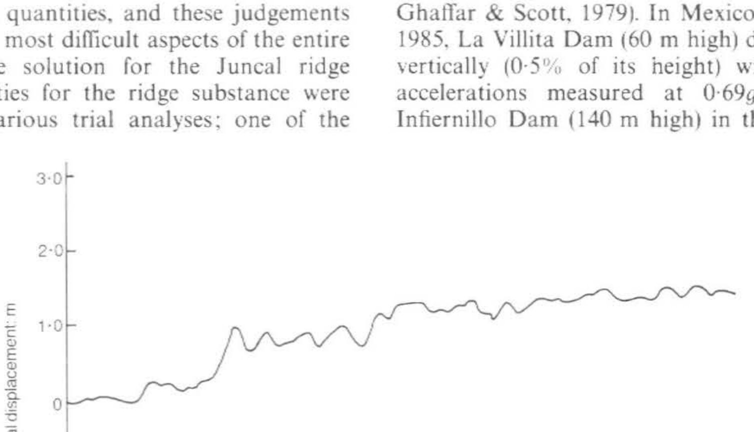


Fig. 28. History of displacement of the crest block of Juncal ridge for a selected base input earthquake acceleration history (after Lindvall, Richter and Associates (1982))

quake had a maximum vertical movement of 0·1 m (0·07% of its height) at accelerations of about $0{\cdot}3g$. Both these dams have experienced several earthquakes and have undergone cumulative horizontal and vertical movements of 0·7 m (1·2%) (La Villita) and 0·45 m (0·3%) (El Infiernillo). For La Villita Dam, the displacements are proportionately larger and have caused concern. The Upper San Fernando Dam (30 m high) displaced about 1 m vertically and horizontally (3% of its height), at peak accelerations of about $0{\cdot}6g$ in the San Fernando earthquake, with clear evidence of a failure mechanism developing in the downstream direction, but without going to completion. Much larger movements occurred in the Lower San Fernando Dam, but it did fail in the structural sense, although no water was released. Pleasant Valley Dam in California, a rolled fill dam, approximately 120 ft high, has experienced shaking from several earthquakes, of magnitudes from 5·7 to greater than 6, producing peak accelerations at the dam site estimated to be as high as $0{\cdot}3g$ with no measured vertical or horizontal displacements of the crest, although minor cracking along the crest has been observed. The displacement was considered sufficiently large at the Juncal ridge to warrant certain rehabilitative measures.

There are other engineering uses in the realm of failure for discrete element computer codes such as those pioneered by Cundall. A few years ago it was realized that there might be earthquake stability problems for statuary at, in particular, the Getty Museum in Los Angeles, or at other museums in seismic areas. At the Getty Museum there were, indeed, hazards. Many of the busts were located on stone plinths which were not anchored to the terrazzo floor. A quick pushing experiment in the absence of a security guard showed that such an arrangement was very fragile. In this case, experiments on a shaking table would be easy to conduct and conclusive, but in their absence it was decided to model the bust and pedestal discretely. The base of the simulated plinth was subjected to a typical Los Angeles design earthquake acceleration input, but this selection was unnecessary since within a second or two of initiation of the event, at accelerations of only a few per cent of gravity, the unfortunate response was apparent (Fig. 29). It is a great pity that some of the few relatively undamaged specimens of Greek sculpture to survive should be at risk in the 20th century in Los Angeles. It is likely that a substantial proportion of missing noses, forearms and legs of Greek marble statues has resulted from seismic events. Incidentally, these figures, in many museums, which lack one or both legs, are sometimes solely supported on a vertical steel bar cemented into a hole drilled in the remaining portion of the leg, and imbedded in a base block. Structurally this is a poor solution to the problem, since it is quite possible, given the period of the lowest mode of vibration of such an arrangement, that, even without overturning, the marble will simply break above the end of the steel bar in the leg, and the statue will fall. Base isolation might be a feasible method of protecting sculptures in seismic areas.

In a different form discrete element models have been employed in studies of the micromechanics of granular media, usually with the grains represented in the form of circular discs, in a two-dimensional simulation. Both static (soil-like) and dynamic (flow) problems have been examined by this approach (Cundall & Strack, 1979; Werner & Haff, 1986; Walton, 1981; Campbell & Brennen, 1985). Some light has been cast on flow processes by the introduction of a pseudotemperature and by the ability to examine at will the mechanisms of momentum transfer in a moving granular mass under conditions where laboratory experiments are difficult if not impossible to perform. When the slope of the flume is small, the mass of grains flows continuously, without gaps (Fig. 30(a)), but in a numerical experiment (Campbell & Brennen, 1985) conducted at a slope angle of 40°, the mechanism of movement became totally different, as a more-or-less intact layer of grains moved at high velocity some distance above the base supported by the impacts of a few grains bouncing backwards and forwards between the base of the layer and the floor of the flume (Fig. 30(c)). Presumably momentum transfer between these particles and the flow provided an equivalent supporting pressure to keep the flow in suspension. In the experiments, no fluid and thus no fluid pressure was included. This might be a possible explanation for the long runout distances of some large landslides, such as illustrated in Figs 15 and 16, for which the air cushion mechanism (Shreve, 1966) is difficult to validate on mechanical grounds.

Attempts so far in the use of micromechanical modelling as a means of elucidating the static mechanical behaviour of granular masses (Cundall & Strack, 1979; Matsuoka, 1983) have not proved fruitful in providing principles to guide, say, the construction of suitable macroscopic constitutive relations. Such calculations are computationally intensive for any reasonable number of grains, even in two dimensions, and most studies have been confined to only a few hundred grains, which is probably not a sufficiently large number statistically. However, the recent introduction of parallel processing computers offers a natural approach to increasing the number of grains involved, and eventually extending the simulation to three dimensions. In

Fig. 29. Calculated response of a bust on a pedestal to earthquake input

such a system several microcomputers (each termed a 'node') are connected together and can pass information among each other. To date, as many as 64 nodes have been assembled. For the granular problem, one node can handle the interactions, say, among 10 grains, informing adjacent nodes when a grain passes in or out of its borders, and the whole system can therefore cover the interactions between 640 grains. Although some housekeeping calculations are required, the computational time is more closely associated with the time required to process 10 grains than that involved in a serial calculation of a mass of 640 grains. Programming at present is difficult, but when such computers become operational and relatively easy to use it may be possible to treat problems of thousands of particles. This obviates questions, such as arose at the Juncal ridge, of the effect of a small number of blocks in the simulation on the final result.

This possibility, when applied to problems such as Juncal Dam, which is an initial attempt at the process to be described, presents a new avenue of approach to the study of the behaviour of granular media at all scales from clay particles to blocks of rocks. Instead of trying to arrive at complex constitutive relations for masses of material consisting of grains or blocks, and subsequently determining for real materials in the laboratory the numerical coefficients (material properties) in the constitutive relations—a difficult task in itself—it might be possible just to simulate the entire granular mass. There are three aspects to this. One is the assumption in such a suggestion that the behaviour at grain contacts and the grain geometry are more important than the properties of the grain itself, and that the properties of granular media in the mass derive substantially from the grain contact forces and movements. Another is that the behaviour of grains at contacts can be adequately modelled. (In this respect clays will present more difficulty than sands or gravels.) The last aspect is the necessity of including a sufficiently large number of grains to be statistically representative of the real material, which will generally consist of many more particles. There should also be sufficient grains in the model that the smallest dimension of any continuous structure (wall, pile, footing), associated with the granular mass, spans many

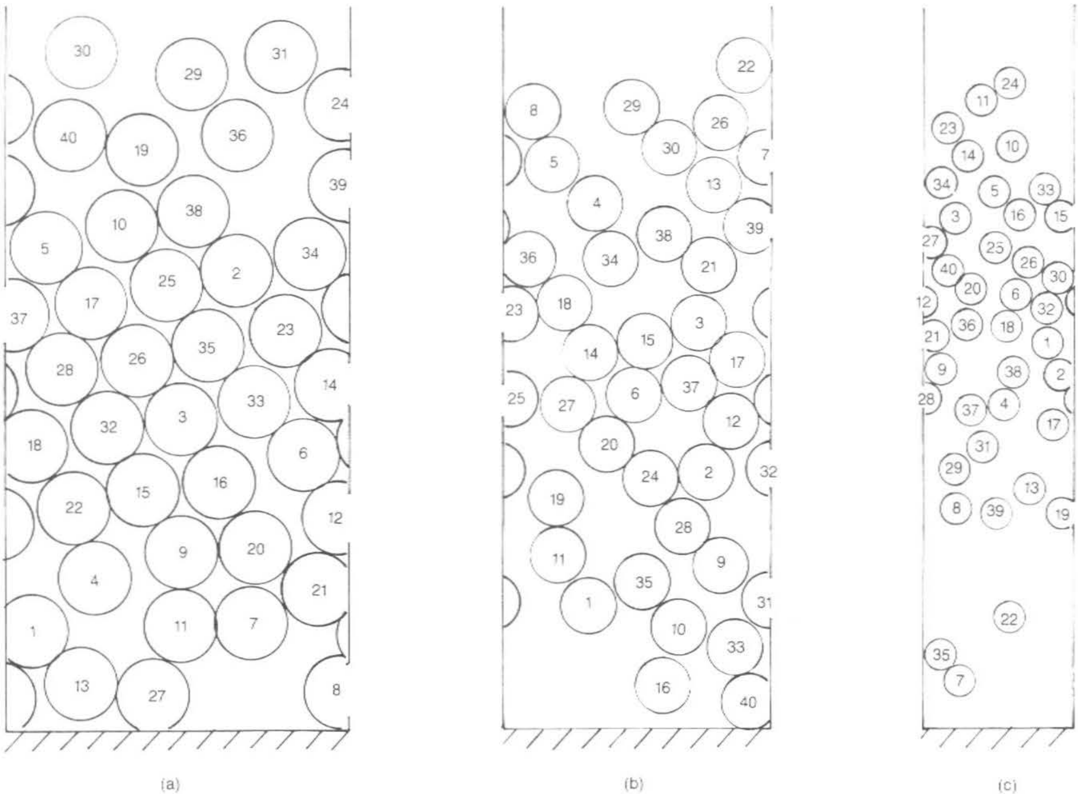


Fig. 30. Computer model of two-dimensional granular medium flow (from left to right) in a sloping chute at various angles (the left- and right-hand boundary conditions are periodic, i.e. a particle emerging from the right-hand boundary is inserted at the same velocity, acceleration etc. through the left-hand boundary) (after Campbell & Brennen (1985)): (a) 20°; (b) 30°; (c) 40°

grains. 'Many' in this case may be 10 or more. If the assumptions involved are valid, many of the problems of constitutive relation formulation and material property determination are avoided, but replaced with questions of grain representation and what happens at the contacts between grains. At least for coarser soil grains, and those minerals which are relatively hard, so no grain crushing occurs, the latter set of difficulties is less imposing than the former. For the less mathematically inclined, the simulation of large displacement processes by the micromechanical approach avoids the difficulties involved with finite deformations in solid mechanics theory, with Lagrangian and Eulerian strains, second Piola–Kirchhoff stress tensors etc., since the equilibrium equations are dealt with at the particle level, and grain displacements follow the kinematic constraints imposed by neighbouring grains.

One example of this approach may be given here. The problem was a two-dimensional one of a single, more massive grain, striking a mass of other similar grains at an impact velocity normal to the surface. Fig. 31 gives a series of snapshots of the penetration process, showing the formation of a crater and ejection of individual grains. Simulations of identical initial particle geometry and contact distribution can be performed, in which, for example, only the mass of individual grains or the intergrain friction or cohesion is altered from test to test. In this way the variation of, say, penetration depth with these properties can be examined. Similar numerical tests have been carried out by Werner & Haff (1986) in their studies of sand grain saltation, an important process in the movement and formation of sand dunes. The effect of the properties cited on the behaviour of a static triaxial test can also be examined (Corkum & Ting, 1986). Eventually three-dimensional models of many grains will bring the simulations closer to real materials. Discrete models at the block level have some utility in the study of, say, mechanisms of deformation and failure of masonry structures, such as arches (Heyman, 1980) and medieval cathedrals (Mark, 1982).

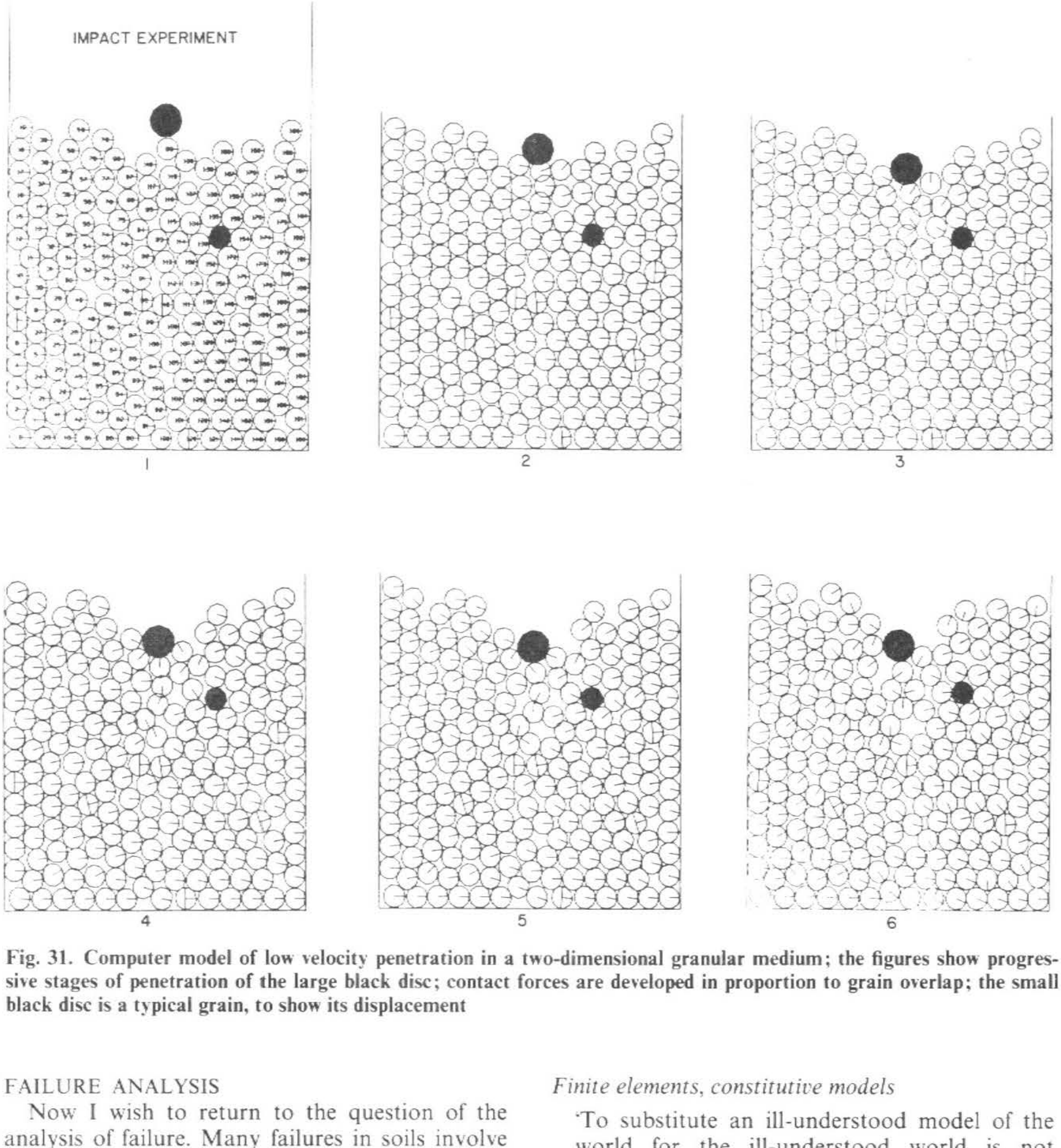


Fig. 31. Computer model of low velocity penetration in a two-dimensional granular medium; the figures show progressive stages of penetration of the large black disc; contact forces are developed in proportion to grain overlap; the small black disc is a typical grain, to show its displacement

FAILURE ANALYSIS

Now I wish to return to the question of the analysis of failure. Many failures in soils involve the development of slip lines or failure surfaces where the material shears, exhibiting displacement discontinuities or shearing zones with widths, in some cases, of only 10 or 20 grain diameters. In other cases, where the material is less dense, failure is associated with extensive zones of shearing as the soil densifies and hardens as it is sheared. These two behaviours were classified by Terzaghi (1943) as 'general' and 'local' failures respectively and are generally thought to be related to unstable and stable material stress–strain responses respectively, although rate dependence is also invoked by metal plasticians (Asaro, 1983; Lemonds, Asaro & Needleman, 1985) as a causative factor.

Finite elements, constitutive models

'To substitute an ill-understood model of the world for the ill-understood world is not progress'—Boyd & Richerson (1986)

A method of analysis, which has evolved over the last 30 years to handle deformation problems in geotechnical engineering, is the finite element methodology. In the linear area of material behaviour, or where the problem conditions permit a reasonable assumption of almost linear response, since no soils are ever linear in their behaviour, the method is clear cut and has been demonstrated (Burland, Simpson & St John, 1979; Casagrande *et al.*, 1972) to give reasonable correspondence with measurements if the relevant material properties can be assessed correctly. More often the finite element analysis indicates the properties which the material must exhibit in the field to give the observed displacements, and how much they differ from test values obtained in the laboratory. When it comes to highly non-linear behaviour of the soil, as it approaches or reaches failure, another situation prevails. Bilinear finite element models were first constructed 20 years ago to represent this condition (Hoeg, Christian & Whitman, 1968) and continue to be used in more sophisticated forms (Smoltczyk, 1982) in the analysis of failure, with various requirements imposed to account for the stress at which the soil yields or fails. Without special treatment, difficulties arise with the matrix operations if the slope of the second part of the curve is made negative, so the bilinear technique has been restricted to stable materials. When this is done solutions are obtained in which the yielded or failure region of the material is usually shown shaded. As successively higher loads are applied to the system, the shaded areas grow and spread. This behaviour may be representative of local failure, consistent with the material property employed. Several examples of this behaviour, taken from a variety of papers, are shown in Fig. 32.

It would be desirable to apply the finite element technique to the problem of slope stability. If a constitutive relation could be used which included a yield or failure condition, then in principle it would appear that, when the proper boundary conditions were applied to a slope which was in a potentially unstable state, a failure region would form in the calculation, in which both kinetics and kinematics were properly accounted for, and the failure mechanism would become evident. At present, with the method, as mentioned before, only stable material behaviour can be included, to obviate numerical difficulties, and the result of an analysis is a picture of a diffuse yielded region, unlike that which develops in the majority of real slope failures, for example. This result appears in finite element analyses of other problems also, such as studies of the bearing capacity of a strip footing. More detailed examination (Burridge, 1987) shows that the development of yielded regions depends on the shape and arrangement of the elements employed. In one study (Prévost & Hughes, 1981) yielding was 'seeded' by including an element in the system that was weaker than adjacent elements, so that it would fail first, and this appeared to give rise to a restricted failure zone, but no application or follow-up to that result appears to have been made. In research on the yielding behaviour of metal specimens in tension at Brown University (Peirce, Asaro & Needleman, 1982; Asaro, 1983) a rate-dependent constitutive relation was proposed and employed in finite element calculations. However, to obtain yielded zones of limited width, another seeding subterfuge was employed by making the boundaries of the problem sinusoidal in shape rather than straight. Two sine functions, of long and short wavelength, were superimposed, the first to give the sample a slight hourglass shape and the second to give rise to local stress concentrations. In a soil mass, much more so than in a metal, there are substantial local variations in soil properties, particularly in terms of strength, and the boundaries of a region are anything but regular. However, the major material and geometrical irregularities control the growth and propagation of a rupture surface in practice, since the principal features of many failures are similar, even in disparate geometries and material properties. Developments in the finite element method will enable the problem encountered in treating unstable material behaviour to be overcome. Advances occur in this area so rapidly that these remarks may soon be outdated.

In the past 10 years there has been a growing interest in the development of constitutive relations to represent the behaviour of soils more realistically than can be obtained by the use of a bilinear elasto-plastic model (Scott, 1985a). Most of these representations involve incremental plasticity, and some of them simulate the overall soil response to loading, or even to a load–unload cycle applied to a soil sample in single-element triaxial or shear tests, quite well. It would therefore have been expected that by this time a variety of finite element programs would have been developed incorporating such material models, and that they would be in wide use in the prediction of soil behaviour in the mass in various two- and even three-dimensional boundary condition problems. Although some codes have been written, they involve incremental plasticity relations of the simpler kind and have been used in only a few studies. In one investigation a cap model was employed in a finite element simulation of the Long Beach, California, subsidence bowl (Kosloff, Scott & Scranton, 1980). Perhaps the earliest complete model is the well-known Camclay model (Schofield & Wroth, 1968), which is a very useful teaching tool in describing the behaviour of normally or slightly overconsolidated clays, when it is applied to the description of a triaxial test. Even with the few constants incorporated in the model, the tracing of a model material's response to a simple loading path applied to a single element is a remarkably complex task, which generally must be handled numerically. The inclusion of this model in a finite element program involves many difficulties (Naylor & Pande, 1981), but it has been implemented (Almeida & Ramalho-Ortigão, 1982)

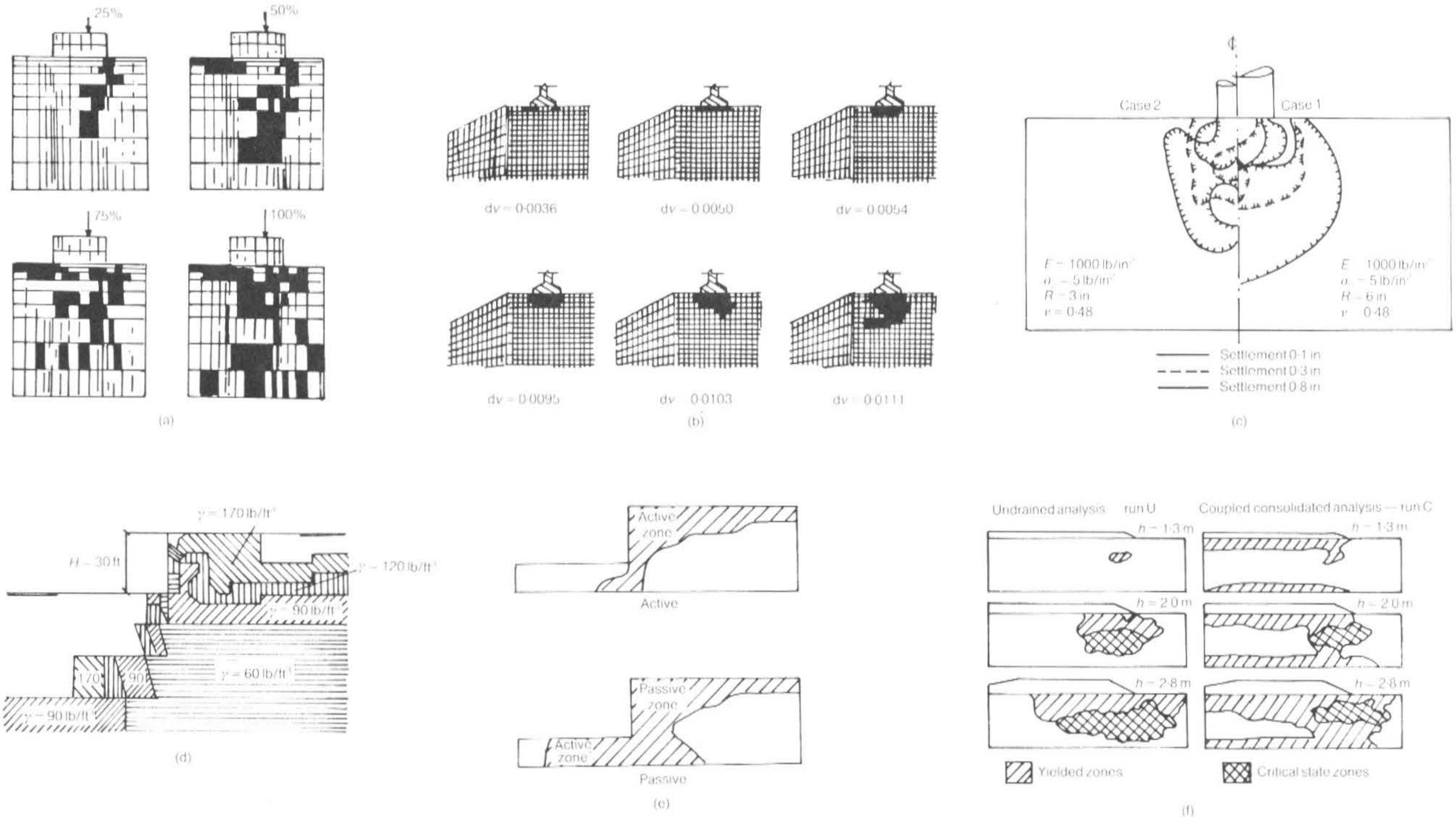


Fig. 32. Finite element solutions for elastic–plastic models (in each case the shaded regions indicate yielding under progressively increasing load or penetration): (a) after Smoltczyk (1982); (b) after Kiousis, Voyiadjis & Tuman (1986); (c) after Yamada & Wifi (1977); (d) after Snitbhan & Chen (1976); (e) after Christian *et al.* (1977); (f) after Almeida & Ramalho-Ortigão (1982) ((c) represents an axially symmetric problem; the other figures represent plane strain problems)

although it is an option in a commercially available program. The bounding surface model has been incorporated in a code (Herrmann, Dafalias & De Natale, 1982) but it has not been widely employed on a variety of soil engineering problems as yet. Many finite element codes using different types of yield surface and flow rules have been described (Christian, Hagmann & Marr, 1977; Davidson & Chen, 1976; Carter, Booker & Davis, 1977; Snitbhan & Chen, 1976). Numerical and analytical solutions to problems associated with pile behaviour have been obtained using the Camclay representation and another model due to Davis and co-workers (Davis, Scott & Mullenger, 1984; Mullenger, Scott & Davis, 1984), but the latter has not yet appeared as a finite element code.

There is no doubt that the coding of these mathematical models is a difficult and trying task; the programmer must have a deep knowledge of the mathematics of the model, as well as of the programming process. In many cases numerical stability problems may be encountered, especially when loading–unloading paths are followed.

When such a code is written, it must be tested, preferably against analytical solutions, but also against field or model test results, before confidence can be gained in its application under all loading conditions. Except for the almost trivial case of linearly elastic response, to which all the models can be reduced, there are *no* analytical solutions against which a numerical solution can be tried. Other difficulties present themselves in the circumstances when experimental data form a basis for comparison. There is the ever-present boundary condition question: do the numerical and field or model test boundary conditions truly correspond? However, the major obstacle has to do with material properties. For either a field or model experiment comparison, the material constants in the constitutive model to be used have to be assessed from conventional laboratory tests. For some models, the number of constants may be as large as 20, and their identification from laboratory tests is a difficult task.

When, as inevitably occurs, the results of the numerical computations differ from the test behaviour, the difference may be due to malfunctioning of the finite element code or may be caused by, say, anisotropic soil behaviour that is not brought out in the single-element laboratory tests. In even the simplest of circumstances, checking the solution provided by such a calculation is a difficult exercise. Schad (1985) presents results showing substantial differences between the results produced by using different algorithms in the same, stable, non-linear finite element code. In addition, none of the constitutive models can currently represent the behaviour of a very dense sand or heavily overconsolidated clay, where failure is accompanied by the development of slip surfaces (Fig. 10). It follows that an associated code lacking these constitutive features, if it existed, could not properly simulate the development of slip surfaces in soils. Some studies (Sture & Ko, 1976; Tarzi, Kalteziotis & Menzies, 1982) have included a strain softening model, but do not discuss resulting displacements or strains. Thus any prototype or model test in which a footing, retaining wall or slope failure (to pick a few examples) occurs with accompanying slip surfaces cannot be represented by a finite element calculation with any of the current models, including the simplest bilinear representation. However, these are frequently the conditions of most interest to a designer. In finite element models, the choice of element and its associated shape function controls the possible deformation modes of the element. For that class of material which exhibits failure surfaces or slip zones during loading, a different calculational approach may be useful.

Dynamic relaxation, finite differences

"When someone says 'I want a programming language in which I need only say what I wish done', give him a lollipop"—Perlis (1982)

For static problems one possibility is the use of the technique called 'dynamic relaxation', in association with the finite difference method. In this approach the computations are set up as for a dynamic problem, but in which the damping is artificial. When a load or displacement is applied, usually as a ramp function in pseudotime, the system responds dynamically with accelerations, velocities and displacements varying in time, but relatively rapidly settles down to a steady state solution. The method is a modification of the original relaxation approach of Southwell (1940) and Cross (1932), but here the relaxation process is controlled automatically by the damping. It seems to have been suggested first by Newmark (1959) but was developed by Day (1965), Otter *et al.* (1966, 1967) and Rushton (1968, 1972) during the 1960s. Chaplin (1971) referred to the procedure as 'metadynamic relaxation'. The term dynamic relaxation seems generally to imply that a finite difference method will be used to solve the associated differential equations. Presumably, a fictitious damping could be introduced with the finite element method also, and could be employed in the solution of non-linear static problems. The advantage of the finite difference method is that it is an explicit technique, and thus does not include matrix operations. Underwood

(1983) indicates also that the convergence rate is slower if finite elements are used. The significant advantage of the dynamic relaxation technique used with the finite difference operation lies in its application to non-linear problems, and particularly those involving unstable material behaviour. It was at one stage a competitor with the finite element method for linear problems, but has diminished in popularity because of the convenience of several finite element features.

In the calculational process, the forces acting on an element are used to calculate the accelerations, from which the velocities and displacements are obtained. The strains can be derived from the displacements of adjacent elements and substituted in appropriate constitutive relations to give the stresses, from which the forces are computed to begin the cycle again. The mass or density and viscosity employed are fictitious parameters that are necessary for the progress of the calculations, since they do not appear in, and do not affect, the final static solution. The direct relation of the stresses to the strains means that non-linear or even unstable stress–strain relations present no difficulties for the numerical process. No matrices are formulated, and the calculation proceeds explicitly step by step through the mesh configuration, one pass for each time step. In the first computations by this method of the deflexions and stresses in concrete dams, for example (Otter *et al.*, 1966, 1967), the material behaviour was assumed to be linear, but the advantages for non-linear response were recognized early and the approach was extended to buckling.

One of the problems that has been referred to frequently in this discussion is that of the stability of slopes. It is an unsatisfactory condition of this situation that an upper-bound solution still has to be obtained by a trial and error method, involving an assumption of a slip surface, followed by an equilibrium analysis of its degree of stability (Bishop, 1955). With all the variations proposed on the details of this approach (Morgenstern & Price, 1965), it is still generally necessary to perform many trials to obtain the worst case. A real worst case mechanism may not be noticed. Basing an analysis solely on the equilibrium of an assumed surface ignores both the constitutive relations of the material and the kinematics of the attendant deformations, as is well known. The variational technique proposed by Baker & Garber (1978), which has not yet been used in routine stability analyses in practice, does generate a failure surface as a result of solving a variational problem.

The finite difference method has recently been adapted by Silling (1986) with the inclusion of finite deformations in a theoretical study of the conditions around a fracture or crack in a plastically deforming material (Abeyaratne, 1981; Knowles & Sternberg, 1978). The equations describing that problem are elliptic if the material behaviour is linearly elastic and become parabolic or hyperbolic (as in soil mechanics) when yielding is encountered. Silling included in his dynamic relaxation finite difference code (CHIMP) a trilinear material behaviour, Fig. 33, of a markedly unstable nature and examined the transition from elliptic through hyperbolic to elliptic response. It seemed that his program might be applied to the problem of interest here: the development of discrete slip surfaces in an unstable material. Having made the necessary adjustments a bar compression test (an unconfined compression plane strain test) was run with the material property given in Fig. 33. The results showed a propagation of slip zones from the corners of the loading plates as displacements increased, until a complete failure mechanism was evident. No weak element or boundary perturbations were necessary, since the rigid end boundary provided a singularity. With further assistance from Silling, a plane strain punch indentation problem has also been examined, with the results shown in Fig. 34 for successive punch displacements. The results of a plane strain punch or bearing capacity test on an essentially half-space region are presented in Fig. 35, along with a granular model test performed several years ago. The technique has been applied (Silling, 1985) to the slope stability problem (Burridge, 1987) where gravity is the loading condition, with the results shown in Fig. 36.

Although this approach needs substantial further development, and needs to be studied in much more detail, it offers the possibility of establishing a failure mechanism in soil mechanics and other structural problems. When the geometry and soil properties of a particular situation (say a slope stability case) have been established, and the problem arranged in the dynamic relaxation

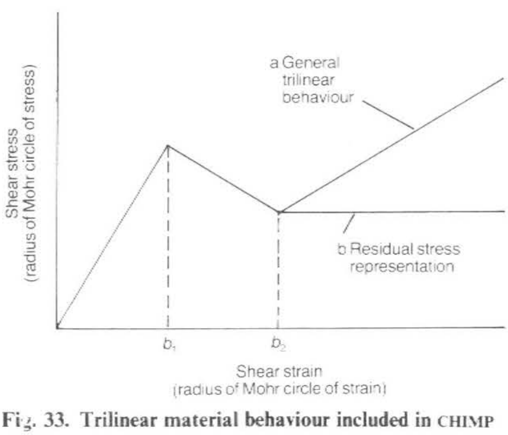


Fig. 33. Trilinear material behaviour included in CHIMP

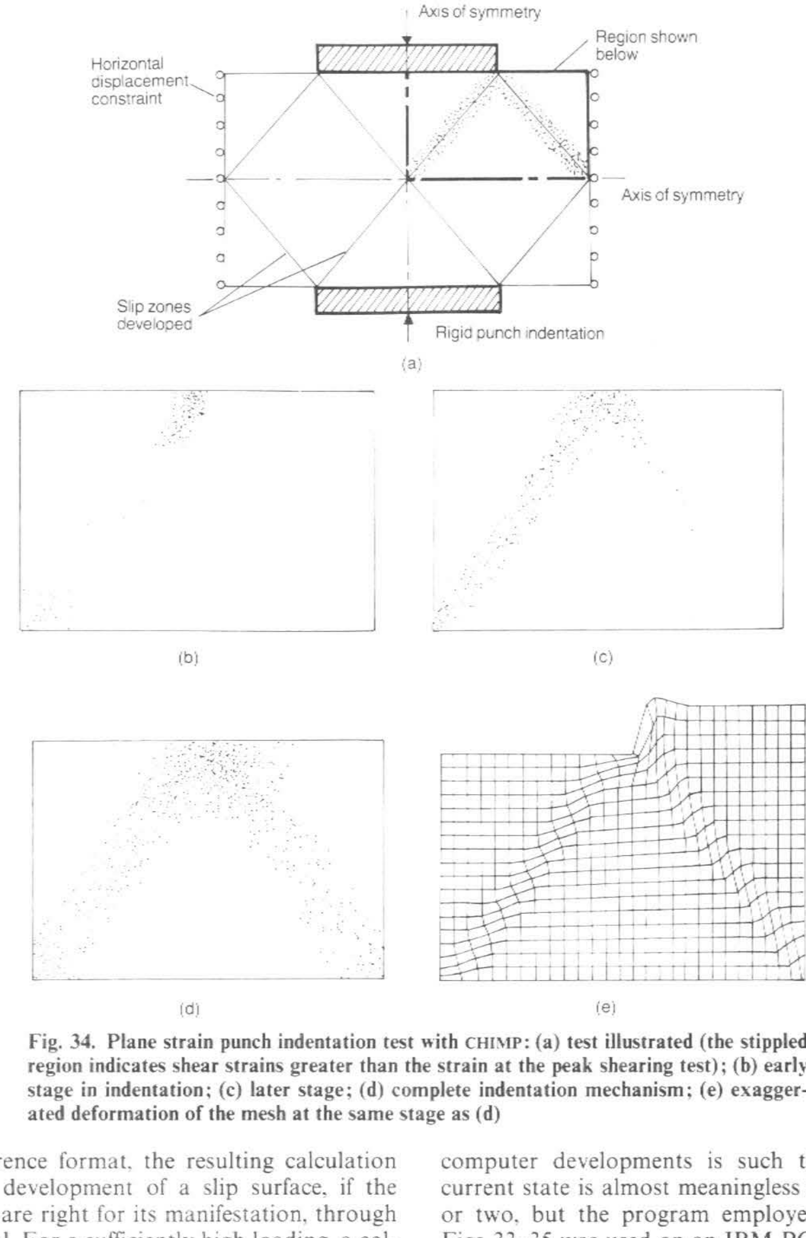


Fig. 34. Plane strain punch indentation test with CHIMP: (a) test illustrated (the stippled region indicates shear strains greater than the strain at the peak shearing test); (b) early stage in indentation; (c) later stage; (d) complete indentation mechanism; (e) exaggerated deformation of the mesh at the same stage as (d)

finite difference format, the resulting calculation traces the development of a slip surface, if the conditions are right for its manifestation, through the material. For a sufficiently high loading, a collapse mechanism will ultimately be generated. At lower loads, a partially penetrating yield zone or no slip surface will develop. For a given load, one computation gives the mechanism, without trial and error, although, of course, another calculation is needed for a different load, to find, say, a failure value. The effect of a different geometry, soil layers or soil properties may be examined. Once a sufficiently high load has revealed a failure mechanism in one analysis, conventional upper-bound analysis can be employed to give a lower upper-bound load. The speed of advance of computer developments is such that to cite a current state is almost meaningless in only a year or two, but the program employed to generate Figs 33–35 was used on an IBM PC AT machine, which required approximately one hour to run 200 time steps of a finite difference network composed of about 600 nodes. The complete development shown in the figures requires 600–700 steps. On a somewhat larger machine (VAX 780) the entire running time is reduced to a few minutes.

CONCLUSION

'It's frightful that people who are so ignorant should have so much influence'—George Orwell (said of V. Gollancz, his publisher)

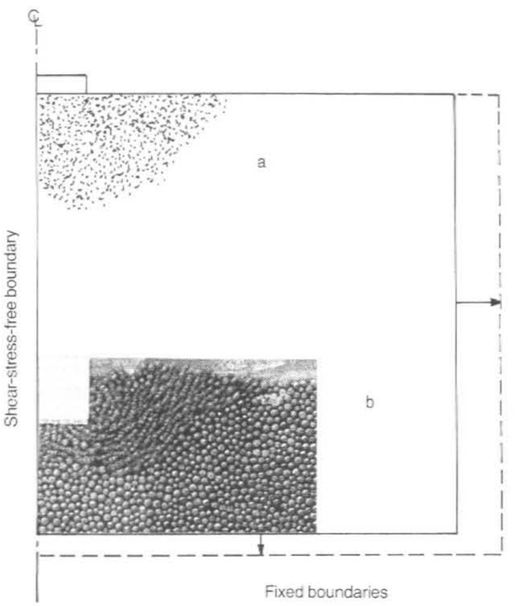


Fig. 35. Plane strain punch test with CHIMP on an infinite region—shear strains in excess of the peak, along with double exposure of a two-dimensional punch experiment (performed in 1969) in a granular medium composed of steel rods: the computational boundaries (broken lines) are more distant than shown

This has been a highly personal view of the question of failure, taken in a broad context, in geotechnical problems, including a description of some field examples, research and a brief exposition of opinions. At the present time, and for many years, many of the usual practical problems in soil engineering have been solved to a level which might be termed economically satisfactory. In looking at the everyday state of practice in terms of static problems, the most recent paper referred to in many commercial soil engineering reports is the well-known one by Bishop (1955) on slope stability analysis! In the area of soil dynamics, progress is still required for practical solutions to seismic conditions, and there are other practical problems, involving soft soils, unusual soils (volcanic or calcareous sediments) and large structures which still need study.

In many areas of soil behaviour, however, intellectual pursuits have been developed which are almost divorced entirely from engineering practice. One example is the mechanics of granular media, in which (Horne, 1965, 1969) an investigation on the behaviour of assemblages of circular discs or spheres can proceed without reference to the deformations of real sand. Another is the study of constitutive relations, although its eventual application to the solution of practical problems is still a goal. Many of the details of this

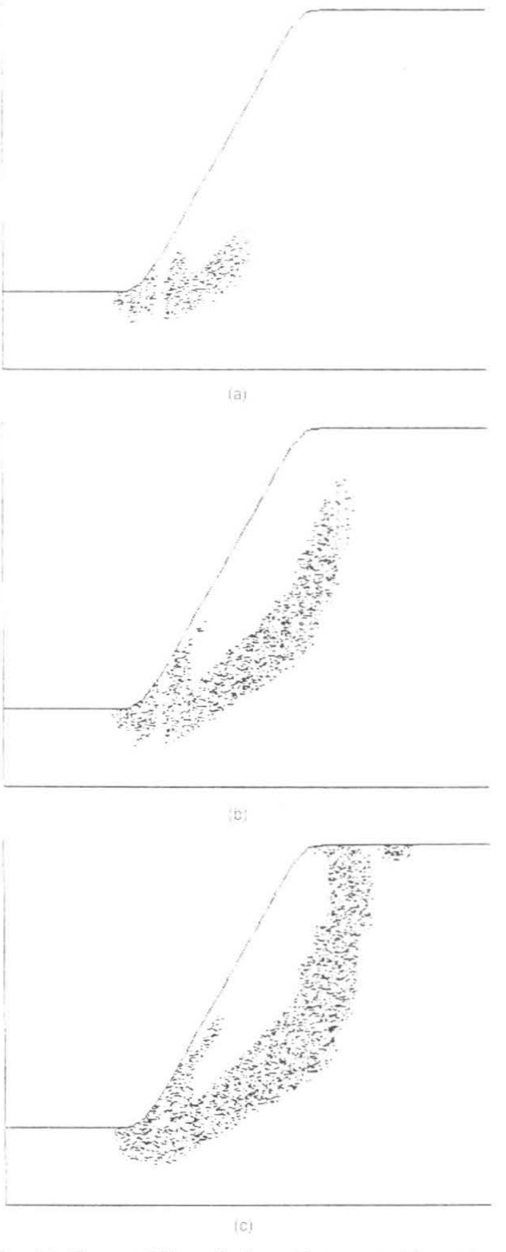


Fig. 36. Slope stability solution with CHIMP with gravitational acceleration exceeding the failure value, applied as a ramp function: (a) excess shear strains at an early stage; (b) at a later stage; (c) final excess shear strain picture (after Burridge (1987))

particular discipline, after the initial solid mechanics foundations were laid a decade or more ago, have reduced to the curve fitting games that are so familiar in traditional soil mechanics and engineering practice. Certain energy conditions give rise to the requirement that the plastic strain increment vector be normal (associative flow rule) to a yield surface, say, but experimental results show that real frictional soils do not know this. Consequently, to fit a particular mathematical model to a real soil response, a non-associative rule is adduced, and the plastic strain increment vector is taken at any angle to the yield surface that fits the experimental results. The angle can be varied as the test proceeds. If such an empirically adjusted model is included in an analytical or finite element solution, the uniqueness of the solution is no longer assured. In various models, both static and dynamic, the shear or bulk modulus variation with applied hydrostatic pressure or shear strain is approximated by some convenient fitting function describing experimental results under some simplified boundary conditions. On occasion a non-linear variation in modulus is approximated by a piecewise linear representation. If the situation is a dynamic one, the calculation is stopped in full flight, all the moduli of different soil elements are switched to new values, and the program is set running again. Clearly the real dissipative plastic nature of the soil response cannot be represented in this way. The use of computers permits almost any variation of properties to be incorporated in an *ad hoc* fashion without regard to energy relations or thermodynamic principles. Is it correct to do this? I am sure that it is not and I feel far from comfortable with the situation.

In any event, the result of a calculation can never been checked. What can it be checked against? It cannot be checked against experience in the field because the necessary measurements of stress, displacement, acceleration etc. are almost never available, or at such few points as to be of little assistance, or, quite properly, the pressure transducers are not to be trusted. Nor can the result be checked against an 'analytical' solution, because they do not exist. I have worried about this problem elsewhere after finding errors in a published slope stability computer program (Scott, 1985b). In examining all computer solutions, what Dawkins (1986) calls 'the principle of personal incredulity' should be continuously employed.

What is left is only judgement, the magic quality of Peck (1981), but in the complex geometries, layering and material properties of the cases that such solutions may be or are applied to, where does judgement enter, and how can it be reliable?

I once had a discussion with Arthur Casagrande, in the general context of the investigation into the failure of the Baldwin Hills Reservoir, on the subject of earth dam design. Talking about design and analysis, he pointed out that I did not grasp the realities of the design situation fully. He said that a mature engineer with experience and judgement (himself), after looking over the site, geology, soil properties etc. very carefully and meticulously, thoroughly absorbing himself in the site conditions, sketched the design on a piece of paper (it might be modified subsequently as more information became available during construction), and it was then drawn up more formally by draughtsmen. The resulting cross-sections, he said 'are then given to young chaps like you to do your analyses on and show that the design is correct'. Calling on the famous remark by Baker (1881) to the effect that experience was based on failure, I asked him about failure of dams that he had designed that way. He said, with some irritation, that none had failed, and I then tried to find out in consequence what the basis of his judgement was that a given dam section, built of a particular soil in a certain valley, subjected to almost unquantifiable, say, seismic risks, was safe. I also tried to enquire why, when, for a large earth dam, every single degree of flattening of the upstream and downstream slopes costs more than US \$1 million dollars, earth dams always come out with slope angles of 2·5:1 or 3:1. I was unsuccessful in obtaining answers to both of these questions, and am not much wiser 16 years later. Little is learned from an unfailed dam, but failures cannot be afforded. Are not more failures needed, in general, in field and model tests, to which analyses can be applied, to ensure that too much money is not being spent unnecessarily? Should not more effort be put into contrived and controlled failures of special structures? Has judgement grown stale when applied to the complex situations that face engineers today?

ACKNOWLEDGEMENTS

The investigations and studies performed by the Author and reported here could not have been carried through without the assistance of many colleagues. It is well known that large teams are engaged particularly in space missions, whose success depends on their efforts. It would not have been possible to have tested the lunar surface without the assistance of many NASA, JPL and Hughes Aircraft Company workers. The Author's immediate colleagues at JPL were F. I. Roberson and M. C. Clary, electrical engineers, who ensured the function and performance of the Surveyor sampler; Roberson and the Author commanded the device on the moon. On Mars, and the Apollo programme, the Author's co-workers are listed on the referenced papers. C. E. Lindvall was kind enough to review the description of the Portuguese Bend landslide, which was discussed frequently together. At Highland Park, former students K. Zuckerman and T.-D. Lu assisted with the instrumentation. M. M. Baligh carried out the finite element fault propagation calculations, and P. B. Burridge set up and ran the Metrorail tunnel experiments on the Caltech centrifuge. The analysis was provided by a colleague, J. Hall, with whom the Author has had many discussions on finite element methods. P. Cundall's distinct element code was modified by J.-P. Bardet, who ran all the simulations in the Juncal ridge study and provided the statue computations. The finite difference computation leading to the slope stability result is a part of research performed by P. B. Burridge.

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VOTE OF THANKS

In proposing a vote of thanks to Professor Scott, Professor R. E. Gibson made the following remarks.

'In the *Proceedings of the Institution of Civil Engineers* for January 1954 there is a discussion contribution by Mr R. F. Scott of Cambridge, Massachusetts, on a paper entitled "Numerical solution of some problems in the consolidation of clay", written by two students of the Institution, Lumb and Gibson. On reading that discussion two things will be discovered: first, how very much further Mr Scott had already gone along this road than the two authors and secondly how clearly his objectives were stated, how well his arguments were marshalled and deployed etc.—in short, just how well it was written.

'This was an early intimation of that talent for clarity of exposition which we have witnessed in the context of a Rankine Lecture. Of course, Professor Scott's wide ranging interests and outstanding competence have given far more than that. This twenty-seventh Rankine Lecture has dealt in a fascinating and stimulating way with failure, a subject of central importance and concern to all geotechnical engineers. Much is learned from the intended failures, and from the history of the discipline it is known that even more may be learned from those which are unintended. In the lecture there has been something for everyone and much to think about.

'On behalf of the British Geotechnical Society I have the honour to propose a hearty vote of thanks to Professor Scott for a most memorable lecture.'